PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCIX.

PART I.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds

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of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the year 1798, two Medals on Sir GODFREY COPLEY's Donation; one to Sir GEORGE SHUCKBURGH EVELYN, Bart. F. R. S. for his various Communications, printed in the Philosophical Transactions; the other to CHARLES HATCHETT, Esq. F. R. S. for his Chemical Communications, also printed in the Philosophical Transactions.

PHILOSOPHICAL TRANSACTIONS.

I. *The Croonian Lecture. Experiments and Observations upon the Structure of Nerves.* By Everard Home, *Esq. F. R. S.*

Read November 8, 1798.

HAVING had the honour of laying before this learned Society several Lectures on the actions of different parts of the organ of vision, the prosecution of the same inquiry has led to some observations on the internal structure of the optic nerve, which will be explained in the present Lecture.

On the first view, the structure of nerves may appear an improper subject; but, when their offices and connection with muscles are maturely considered, any knowledge respecting them will be allowed an important acquisition towards the investigation of muscular motion.

In bringing forward an account of newly-acquired facts, the most natural, and therefore the most satisfactory method is, to begin with the circumstances which led to their detection. This at present becomes the more proper, as the experiments which brought the subject of nervous structure under consi-

deration, were made upon the eye, and were in some measure connected with the observations contained in the former Lectures: they were instituted with a view to ascertain the cause of the luminous appearance frequently observed in the cat's eye.

The illumination so conspicuous in the eye of the cat, and of many other animals, when seen in an obscure light, has attracted the attention of every common observer. Philosophers also have paid particular attention to it, and have endeavoured to investigate the cause. On this subject there have been two opinions: one, that the illumination arises from the external light collected in the eye, and reflected; the other, that there is a quantity of light generated in the organ itself.

Professor BOHN, at Leipsick, made experiments which proved, that when the external light is wholly excluded, none can be seen in the cat's eye.

These experiments were favourable to the first opinion; but the brightness of the illumination is so great, that it appeared to exceed any effect which could be produced through the medium of the retina; so that some other source of light was thought necessary to account for the phænomenon: this circumstance gave support to the second opinion.

To determine which of the two opinions was just, several experiments were instituted, under the direction of Mr. RAMSDEN, who likewise assisted in making them. The truth of Professor BOHN's experiments was readily ascertained; it therefore only became necessary to inquire, whether the external light was of itself capable of producing so great a degree of illumination as that seen in the cat's eye.

This was attended with difficulty; for, when the apartment

in which the experiments were made was so much darkened that nothing but the illumination from the eye was visible, the animal, by change of posture, or some other means, almost immediately deprived the observers of all light from that source. This was found to be the case, whether the cat, the tiger, or the hyena, was the subject of the experiment. On the other hand, when the light in the room was sufficient for the animal itself to be seen, the illumination in the eye was more obscure, and appeared to arise from the external surface of the iris.

As the difficulties which occurred in making observations on the illuminated state of the eye in the living animal were so great, an attempt was made to repeat, as nearly as possible, the experiments after death.

In doing so it was found, that a strong light thrown upon the cornea illuminated the iris, as it had done in the living eye; but, when the cornea was removed, this illumination disappeared. The iris was then dissected off, and the lucid tapetum completely exposed to view; the reflection from which was extremely bright; the retina proving no obstruction to the rays of light, but appearing equally transparent with the vitreous humour and crystalline lens.

From these experiments it appeared evident, that no light is generated in the eye; the illumination being wholly produced by the concave bright-coloured surface of the tapetum, collecting the rays of the external light, concentrated by the cornea and crystalline lens, and reflecting them through the pupil. When the iris is completely open, the degree of brilliancy is the greatest; but, when the iris is partly contracted, which it always is when the external light is increased, then the illumination is more obscure, and appears to come from the

iris; a part of the light reflected from the tapetum being thrown back, by the concave surface of the cornea, upon the anterior surface of the iris, giving it a bright shining appearance.

The influence which the will of the animal has over this luminous appearance, seems altogether to depend on the contraction and relaxation of the iris. When the animal is alarmed or first disturbed, it naturally dilates the pupil, and the eye glares; when it is appeased or composed, the pupil contracts, and the light in the eye is no longer seen.

The most material information that has been gained in this investigation, is the transparent state of the retina in the eye during life; the opaque membranous appearance which it puts on in the dead body not being natural to it, but a change which takes place in consequence of death. This fact is almost all that is necessary, to explain the luminous appearance in the eyes of cats.

That neither Baron HALLER nor FONTANA had an adequate idea of the transparency of the retina, will appear from the following expressions respecting it, taken from their works:

HALLER describes it in the following words,

"Membranam crassam quidem, sed mollisimam, pellucidam "utique, quando recens oculus inspicitur, ut per eam sub aquis "choroideam videas; tamen ex flavo subcineream."* So that, although he calls it transparent, he says it is of a yellowish ash-colour.

FONTANA's expressions are, "Cette insensibilité de la rétine "à la lumière, en tant que lumière, dérive-t-elle de ce que les "nerfs sont encore trop gros, et ne sont pas bien découverts des

* *Elementa Physiologiæ*, Tom. V. p. 385.

" tissus cellulaires? ou de ce que la pulpe de la rétine est trop " amoncelée, et empêche les rayons de lumière d'arriver jusqu'à " ces mêmes nerfs?" *

In considering the use of the lucid tapetum, it was an idea of the late Mr. HUNTER's, that the retina received a double stroke from the rays of light which entered the eye; one in passing to the tapetum, the other in returning from it.

This very ingenious opinion had some difficulties opposed to it, while the retina was supposed capable of obstructing the rays of light even in the smallest degree, as they could not be equably transmitted, so as to affect every part of the membrane alike. But the retina being ascertained to be absolutely transparent, these objections are entirely removed, and there can be no doubt that the rays of light, in those eyes which have a lucid tapetum, must remain upon the retina as long again as in the eyes of other animals; since the time required to strike upon the tapetum, and return, must be twice as much as is necessary for passing through the retina, to reach the nigrum pigmentum, where they are lost.

This may appear to be a consideration of little consequence, as the velocity of light is so great, and the continuance of impression necessary for distinct vision is that produced by a successive flow of similar rays of light from the object; it may, however, be all that is necessary for the purpose.

The retina being found perfectly transparent, when the eye is examined in a recent state, led to the idea that the internal structure of the optic nerve, when examined in the same state, might also be transparent. To ascertain this point, the following experiment was made:

* *Sur le Venin de la Vipere*, 1781. Vol. II. p. 219.

The posterior half of a cat's eye, while in a very recent state, was immersed in a bason of water, and examined. The tapetum appeared very bright, the retina not having acquired sufficient opacity to become visible: the entrance of the optic nerve was a very white spot, which seemed to be opaque; but, when small pieces of coloured paper were alternately placed between the outside of the eye and the bottom of the bason, their colour was distinctly seen in the cavity of the eye, through the substance of the optic nerve; so that, at this part, the internal structure of the nerve has a degree of transparency.

This appeared to be a newly-discovered fact; and, to ascertain whether it was really so, the works of several physiological writers were consulted, but nothing was found which gave an idea that their authors had the smallest knowledge of it.

This semi-transparent state of the internal parts of the optic nerve, while recent, led naturally to the examination of its substance, by means of magnifying glasses; and, notwithstanding the failure of so many men of superior abilities, in this intricate inquiry, it held out the hope of meeting with some success.

The principal theories which have been formed respecting the structure of nerves, have been taken notice of by FONTANA: as they all differ from the observations which will be stated in the present Paper, it may not be improper to mention the heads of each of them, so as to bring into one point of view, all the knowledge that has been acquired on the subject.

TORRE found the medullary substance of the brain, spinal marrow, and nerves, to be a mass of transparent globules, swimming in a transparent fluid. When the parts were magnified one thousand times, the globules appeared largest in the

brain, and smaller in the spinal marrow; they had no regular order: but, in the nerves, the globules were placed in lines, so as to give the appearance of fibres. In examining the optic nerve, the parts were magnified 120 times.

PROCHASKA considered the nerves to be composed of globules, united by a transparent elastic cellular membrane, and disposed in straight lines, resembling fibres.

FONTANA found the primitive structure of nerves to consist of transparent cylinders, which, when united, formed the nerve: the manner of their being disposed is not mentioned. The objects were magnified 700 times, to show this appearance.

Dr. MONRO considered the nerves as made up of spiral fibres; but afterwards found that what he had described was entirely an optical deception. In his last work, he says, "The optic "nerves have, in their whole course, less appearance of a fibrous "structure than perhaps any other pair of nerves in the human "body."

Other authors may have written upon this subject, and may have made observations upon the structure of nerves, but want of leisure must be an excuse for my not having come to a knowledge of them.

It is scarcely necessary to mention, that parts of an animal body are not fitted for being examined by glasses of a great magnifying power; and, wherever they are shewn one hundred times larger than their natural size, no dependance can be placed upon their appearance.

In making the following microscopical experiments on the internal structure of the optic nerve, great care was taken to avoid the errors of former inquirers. The microscope used was a single one; the focal length of the lens was about $\frac{1}{100}$

of an inch, so that the object was magnified about 23 times; and, that the results of the experiments might be as free from optical deceptions as the present state of our knowledge in this branch of science will admit, no appearance is described which Mr. RAMSDEN was not satisfied of having distinctly seen.

The experiments performed with the single microscope were repeated with a double one, made by Mr. RAMSDEN, which magnified the object about 40 times; but, in the double microscope, the appearances were indistinct, the reflection from the different glasses having thrown a confused glare upon the moist surface of the nerve. This circumstance led Mr. RAMSDEN to object to the use of compound microscopes, and to consider them as unfit for viewing objects of this kind.

For the following reasons, the optic nerve of the horse was selected, as the most proper for the experiments. It is of a large size, and several inches in length. It is readily procured in a recent state; as there are places in London where horses are allowed to be killed, and regular days in the week are fixed for that purpose.

That the examination of the nerve might be made as soon as possible after the animal's death, permission was procured from the man who superintends the killing of horses, to allow Mr. CLIFT to make the necessary experiments on the spot, the moment the horses were killed. Mr. CLIFT is the person intrusted with the care of keeping in order the late Mr. HUNTER's collection in comparative anatomy, and is well qualified, from his anatomical knowledge, and a familiarity in looking at organized parts through magnifying glasses, for an examination of this kind. These experiments were afterwards repeated by Mr. RAMSDEN and myself. From this mode of conducting

them, the chances of error were few; since the person who first observed the appearances had no previous opinions on the subject; and Mr. RAMSDEN was better able than any other person, to correct such optical errors as might deceive Mr. CLIFT or myself.

The first experiments were made upon transverse sections of the nerve. One, near its termination in the eye, was placed upon glass, and exhibited in the microscope the following appearances: it was evidently composed of two parts, one opaque, the other transparent. The opaque portions were nearly circular in their shape, about 600 in number, and touched one another; the interstices between them were transparent.

When the opaque parts were attentively examined in a favourable light, and the nerve was in a recent state, they were found to be made up of a great number of smaller portions, each of which appeared to be also opaque. To see this subdivision of parts required some attention, and in many sections it could not be perceived. The cause of the difficulty seemed to be, the softness and tenacity of the substance divided, which therefore spread itself over the surface, giving it an uniform appearance; but, towards the circumference of the nerve, where the parts were cut obliquely, and some of them torn, the subdivision was very distinct. It was first observed by Mr. CLIFT, in several different sections; and was afterwards seen very distinctly, both by Mr. RAMSDEN and myself, in a nerve examined about two hours after death.

Having repeated these experiments six or seven times, on different days, so as to ascertain the accuracy of the results, the next object was, to determine whether the nerve had the same structure in its whole course. For this purpose, trans-

verse sections were examined in different parts of the nerve, near the brain, towards the middle, and nearer the eye: of these experiments the following are the results. In all the sections, the nerve appeared to be made up of the same substances; but the size and number of the opaque parts differed very much. They have been stated, near the eye, to be 600; about the middle of the nerve, they were 150; and, near the brain, between the origin and union of the two nerves, they were only about 40. As they became larger, they were less regular in their shape, and had less of a circular form; nor were they uniform, some appearing very large, with one or two smaller placed between them.

After having succeeded in this examination of the nerve transversely, an attempt was made to investigate its structure in a longitudinal direction. To do this, a portion of the nervous pulp had its coat, formed by the dura mater, along with a thin vascular membrane which lines it, carefully removed for about an inch in length: the external surface of the pulp was then examined with a magnifying glass; the structure was evidently fasciculated, but the fasciculi did not run parallel to one another; they seemed to unite together and separate again, in such a manner that any one of them could not be traced for half an inch in length, without being lost in the neighbouring part. When thin sections were examined in the field of the microscope, they put on the same appearance: this was equally the case, whether the part examined was near the centre or circumference of the nerve. The fasciculi were largest in that part of the nerve near the brain, and smallest towards the eye. Great pains were taken to ascertain whether the fasciculi were made up of continued fibres, or of small parts unconnected,

which, from their position, gave that appearance; but every observation that was made was in proof of their being continued fibres.

From these experiments, the internal structure of the optic nerve appears to be made up in the following manner:

At its origin from the brain, it consists of thirty or forty fasciculi or bundles of extremely small opaque pulpy fibres, the interstices between which are filled with a transparent jelly. As the nerve goes farther from the brain, the fasciculi form smaller ones, of different sizes. This is not done by a regular subdivision, but by a few fibres going off laterally from several large fasciculi, and being united, forming a smaller one: some of the fasciculi so formed, which are very small, unite again into one. In this way, the fasciculi gradually diminish in size, and increase in number, till they terminate in the retina.

Near the eye, where the fasciculi are most numerous, the substance of the nerve has a considerable degree of transparency, from the number of transparent interstices between them; but this is less the case nearer the brain, where the interstices are fewer.

In the optic nerve of the cat, the structure is the same as in the horse; but, from the smallness of the parts, less fitted for investigation. Near the eye, its internal substance is more transparent than the corresponding part in the horse.

To see how far this structure was peculiar to the optic nerve, similar experiments were made upon the internal substance of the fifth and seventh pair of nerves, near their origin at the brain, and the structure was found to be the same. In these last mentioned nerves, the interstices between the fasciculi were smaller than in the optic nerve, rendering their transverse

sections less transparent; from which it is natural to suppose, that the internal parts of the optic nerve are not so compact as in other nerves, and therefore it is better fitted for examination.

These experiments show, that the nerves do not consist of tubes conveying a fluid, but of fibres of a peculiar kind, different from every thing else in the body, with which we are acquainted. The course of these fibres is very curious; they appear to be constantly passing from one fasciculus to another, so as to connect all the different fasciculi together by a mixture of fibres. This is different from the course of blood-vessels, lymphatics, or muscular fibres: the only thing similar to it, is in the formation of nervous plexuses; which leads to the idea of its answering an essential purpose, respecting the functions of the nerves.

II. *The* Bakerian *Lecture. Observations upon an unusual horizontal Refraction of the Air; with Remarks on the Variations to which the lower Parts of the Atmosphere are sometimes subject. By the Rev.* S. Vince, *A. M. F. R. S. and Plumian Professor of Astronomy and experimental Philosophy, in the University of Cambridge.*

Read November 15, 1798.

THE uncertainty of the refraction of the air near the horizon has long been known to astronomers, the mean refraction varying by quantities which cannot be accounted for from the variations of the barometer and thermometer; on which account, altitudes of the heavenly bodies which are not more than 5° or 6°, ought never to be made use of when any consequences are to be deduced from them. The cause of this uncertainty is probably the great quantities of gross vapours, and exhalations of various kinds, which are suspended in the air near to the earth's surface, and the variations to which they are subject; causes, of which we have no instruments to measure the effects which they produce, in refracting the rays of light. In general, the course of a ray passing through the atmosphere, is that of a curve which is concave towards the earth, the effect of which is to give an apparent elevation to the object; and thus the heavenly bodies appear above the horizon, when they are actually below it; but it will not alter the position of their parts, in respect to the horizon,

that is, the image of the highest part of the object will be uppermost, and the image of the lowest part will be undermost. The figures, however, of the sun and moon, when near the horizon, will suffer a change, in consequence of the refraction of the under limb being greater than that of the upper; from which they assume an elliptical form, the minor axis of which is perpendicular to the horizon, and the major axis parallel to it. But a perpendicular object, situated upon the surface of the earth, will not have its length altered by refraction, the refraction of the bottom being the same as that of the top.* These are the effects which are produced upon bodies at or near the horizon, in the common state of the atmosphere, by what I shall call the *usual* refraction.

But, besides the usual refraction which affects the rays of light, the atmosphere over the sea is sometimes found to be in a state which refracts the rays in such a manner as to produce other images of the object, which we will call an effect from an *unusual* refraction. In the Phil. Trans. for 1797, Mr. HUDDART has described some effects of this kind, which he has accounted for by supposing that, from the evaporation of the water, the refractive power of the air is not greatest at the surface of the sea, but at some distance above it; and this will solve, in a very satisfactory manner, all the phænomena which he has observed. But effects very different from those which have been described by Mr. HUDDART are sometimes found to take place. These I had an opportunity of observing at Ramsgate, last summer, on August the first, from about half an hour after four o'clock in the afternoon till between seven and eight. The day had been extremely hot, and the evening was very

* See my *Complete System of Astronomy*, Art. 194.

sultry; the sky was clear, with a few flying clouds. I shall describe the phænomena as I observed them with a terrestrial telescope, which magnified between 30 and 40 times; they were visible, however, to the naked eye. The height of the eye, above the surface of the water, at which most of the observations were made, was about 25 feet; some of them, however, were made at about 80 feet from the surface; and it did not appear that any of the phænomena were altered from varying the height of the eye, the general effect remaining the same.

The first unusual appearance which I observed, was that which is represented in Tab. I. fig. 1. Directing my telescope at random, to examine any objects which might happen to be in view, I saw the top of the masts of a ship A, above the horizon, *x y*, of the sea, as shown in the figure; at the same time also, I discovered in the field of view, two complete images, B, C, of the ship in the air, vertical to the ship itself, B being inverted, and C erect, having their hulks joined. The phænomenon was so strange, that I requested a person present to look into the telescope, and examine what was to be seen in it, who immediately described the two images, as observed by myself; indeed they were so perfect, that it was impossible we could differ in our description. Upon this, I immediately took a drawing of the relative magnitudes and distances of the ship and its images, which, at that time, were as represented in the figure, as near as it was possible for the eye to judge; and it was very easy to estimate them to a very considerable degree of accuracy. As the ship was receding from the shore, less and less of its masts became visible; and, continuing my observations, in order to discover whether any, or what variations might take place, I found that, as the ship descended, the images B, C

ascended; but, as the ship did not sink below the horizon, I had not an opportunity of observing at what time, and in what order, the images would have vanished, if the ship had so disappeared.

Being desirous of seeing whether the same effect was produced upon the other ships which were visible, I directed my telescope to another ship A, (fig. 2.) whose hulk was just in the horizon xy; when I observed a complete inverted image B, the main-mast of which just touched that of the ship itself. In this case, there was no second image as before. The ship A moving upon the horizon, B continued to move with it, without any variation in its appearance.

The next ship which I directed my telescope to, was so far on the other side of the horizon xy, as just to prevent its hulk from being seen, as is represented by A, (fig. 3.). And here I observed only an inverted image of part of the ship; the image y of the topsail, with the mast joining that of the ship, the image x of the top a of the other mast, and the image z of the end c of the bowsprit, only appearing at that time. These images would suddenly appear and disappear very quickly after each other; first appearing below, and running up very rapidly, showing more and less of the masts at different times, as they broke out; resembling, in the swiftness of their breaking out, the shooting out of a beam of the Aurora borealis. As the ship was descending on the other side of the horizon, I continued my observations upon it, in order to discover what changes might take place; when I found, that as it continued to descend, more of the image gradually appeared, till at last the image of the whole ship was completed, with their main-masts touching each other; and, upon the ship descending lower, the image

and the ship separated; but I observed no second image, as in the first case; a second image, however, might probably have appeared, if the ship had continued to descend.

Upon moving my telescope along the horizon, in order to examine any other ships which might be in sight, I observed, just at the horizon *x y*, (in fig. 4.) the top *a* of the mast of a ship; and here an effect was observed, which had not been before discovered; for there was an inverted image B, vertical to *a*, an erect image C, both of them very perfect and well defined, and an image *v w* of the sea between them, the water appearing very distinctly. As the ship was coming up towards the horizon, I continued to observe it, in order to discover the variations which might follow, and found, that as the ship approached the horizon, the image C gradually disappeared, and at last it vanished; after that, the image *v w* of the sea disappeared; and during this time the image B descended; but the ship did not rise so near to the horizon as to bring the main-masts together. Had I directed my telescope to the same point of the horizon a little sooner, I should have seen the two images, before the ship itself was visible. In fact, the images were visible, when the whole ship was actually below the horizon; for, from the very small part of the mast which was at first visible, that part must then have been below the horizon, and appeared above it by the usual refraction; the altitude of *a*, above the horizon, having then been much less than the increase of altitude which arises from the common horizontal refraction. The discovery of ships in this manner might, in some cases, be of great importance; and, on such occasions, it might be worth while to appoint proper persons to make observations for that purpose.

The cliffs at Calais being very visible, I directed my telescope towards them, in order to examine whether there was any thing unusual in their appearance; when I observed an image of the cliffs, above the cliffs themselves, together with an image of the sea separating them, as is represented in fig. 5.; in which, *x y* represents the horizon of the sea, AB the cliffs, *a b* their image, and *v w* the image of the sea between them: the depth of *a b* was much less than that of AB. It is probable, however, that *v w* might not be the image of the sea immediately adjoining to the cliffs, but a partial elevation of the sea at some distance from them; and that the image *v w* might intercept some part of the image *a b*, which would otherwise have been visible: we must not therefore conclude, that the image *a b*, so far as it appeared, was less than the corresponding part of the object. From the memorandums which I made at the time of observation, I do not find that I examined the appearance of the cliff AB, and its image *a b*; which, had there at that time been any striking marks in them, would have determined whether the object and its image were of the same magnitude. The image *a b* was, however, erect; the boundaries on the top of AB and *a b* agreeing together. Having examined this for some time, and taken a drawing of the appearance, during which I could discover no variation, I directed my telescope to other objects; and, upon turning it again to the same cliffs, after the space of about six or seven minutes, the images *a b* and *v w* were vanished; but, examining them again soon after, the images were again visible, and in every respect the same as they appeared before. A short time after, they disappeared, and did not appear any more.

Soon after the above appearances, I observed a ship C, with

the hulk below the horizon *x y*, passing by the same cliffs AB; an inverted image D of which appeared against the cliffs, as represented in fig. 6. The ship was in motion, and remained at the same distance on the other side of the horizon: I continued my observations upon it till it had passed the cliffs for a considerable distance, but there was no change of appearance. The cliffs were illuminated by the sun, and appeared very distinctly; but there was no image above, as in the last case.

Continuing to observe the same cliffs AB, fig. 7, I soon after discovered two partial elevations *m*, *n*, of the sea, by the unusual refraction; they changed their figures a little, and disappeared in the place where they first appeared, and were equally distinct in every part.

About this time, I observed a very thick fog coming upon the horizon from the other side, rolling upon it with a prodigious velocity; curling as it went along, like volumes of smoke sometimes out of a chimney. This appeared several times. I conclude, therefore, that there was a considerable fog on the other side of the horizon.

The last phænomenon which I observed was that which is represented in fig. 8; where *x y* represents the horizon, *a b* two partial elevations of the sea, meeting at *c*, and continued to *d*; *e*, another partial elevation of the sea, of which kind I observed several, some of which moved parallel to the horizon, with a very great velocity. I conjecture, therefore, that these appearances were, in part at least, caused by the fog on the other side of the horizon. For, though I did not at the same time see the motion of these images and that of the fog, yet, from memory, I judged the motions to be equal; and they were also in the same direction. A fog which, by producing an unusual refrac-

tion, might form these images, would, by its motion, produce a corresponding motion of the images.

I have here described all the different phænomena which I observed from the unusual refraction, of most of which I saw a great many instances. Every ship which I observed on the other side of the horizon of the sea, exhibited phænomena of the kind here described, but not in the same degree. Of two ships which, in different parts, were equally sunk below the horizon, the inverted image of one would but just begin to appear, whilst that of the other would represent nearly the whole of the ship. But this I observed, in general, that as the ship gradually descended below the horizon, more of the image gradually appeared, and it ascended; and the contrary, when the ships were ascending. Upon the horizon, in different parts, one ship would have a complete inverted image; another would have only a partial image; and a third would have no image at all. The images were in general extremely well defined; and frequently appeared as clear and sharp as the ships themselves, and of the same magnitude. Of the ships on this side of the horizon, no phænomena of this kind appeared. There was no fog upon our coast; and the ships in the Downs, and the South Foreland, exhibited no uncommon appearances. The usual refraction at the same time was uncommonly great; for the tide was high, and at the very edge of the water I could see the cliffs at Calais a very considerable height above the horizon; whereas they are frequently not to be seen in clear weather from the high lands about the place. The French coast also appeared both ways, to a much greater distance than I ever observed it at any other time; particularly towards the east, on which part also the unusual refraction was the strongest.

During the remainder of my stay at Ramsgate, which was about five weeks, I continued daily to examine all the ships in sight; but I discovered no phænomena similar to those which I have here given a description of. The phænomenon of the ship observed by Mr. HUDDART, differed altogether from those above described, as the inverted image which he observed was below the ship itself. An appearance of this kind I observed on August the 17th, about half an hour after three o'clock in the afternoon, of which fig. 9. is a representation. The real ship is represented by A, and the image by B; *er*, *mv*, the hulks; *st* the flag, and *wx* its image, just touching it, with the sea *xy* below. Between the two hulks, some faint dark spots and lines appeared, but I could not discover what they were the representatives of. The vessel, at the time of this appearance, was not quite come up to the horizon; and, as it approached it, the image gradually diminished, and totally disappeared when the ship arrived at the horizon.

It remains now, that we inquire into the causes which might produce the very extraordinary effects which have been above related. From the phænomena, we are immediately led to the nature of the path of the rays of light to produce them; and we may conceive, that the air may possibly be in such a state as will account for the unusual tract which they must have described. For, let *bx* (fig. 10.) be the surface of the sea; *ab* an object; E the place of the eye; *ar* E, *bs* E, the progress of two rays, by the usual refraction, from the extreme parts of the object to the eye; to these curves draw the tangents E *a'*, E *b'*, and *a' b'* will be the image of the object, as usually formed. Now, if we take the case represented in fig. 4, let *a''* *b''* represent the inverted image, and *a'''* *b'''* the erect image; join *a''* E,

a''' E, and b'' E, b''' E, and these lines must respectively be the directions of the rays entering the eye from a and b, in order to produce the images a'' b'' and a''' b'''; hence, these lines must be tangents at E, to the curves which are described by the rays of light; let therefore $a\,n$ E, $a\,m$ E, $b\,v$ E, $b\,w$ E, be the curves described. We have therefore to assign a cause which may bring rays passing above the rays $a\,r$ E, $b\,s$ E, to the eye at E. Now, if there were no variation of the refractive power of the air, a ray of light passing through it would describe a straight line; therefore, the curvature of a ray of light passing through the atmosphere, depends upon the *variation* of the refractive power of the air. If, therefore, we suppose the air lying above $a\,r$ E, to vary quicker in its refractive power than the air through which $a\,r$ E passes, the curvature of a ray proceeding above that of $a\,r$ E, will be greater than the curvature of $a\,r$ E; and upon this principle we may conceive that a ray may describe the curve $a\,n$ E: and, in like manner, if a quicker variation of refractive power should take place above the curve $a\,n$ E, than in that curve, a third ray may describe the curve $a\,m$ E. The same may be said for the rays $b\,v$ E, $b\,w$ E, diverging from b. The alterations of the refractive power may arise, partly from the variations of its density, and partly from the variations of its moisture; and the passage of the rays through the boundary of the fog may there suffer a very considerable refraction; for, from the motion of the fog, and that of the images abovementioned, I have no doubt that the fog was a very considerable agent in producing the phænomena. When all the causes co-operate, I can easily conceive that they may produce the effects which I have described. If the cause should not operate in the tract of air through which the curves

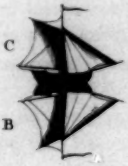

Fig. 1.

Fig. 2.

Fig. 5.

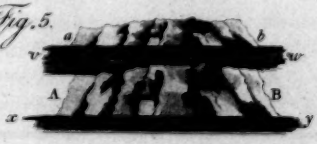

Fig. 6.

Fig. 7.

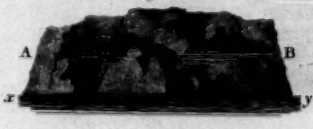

Fig. 8.

Fig. 3.

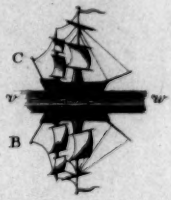

Fig. 4.

Fig. 9.

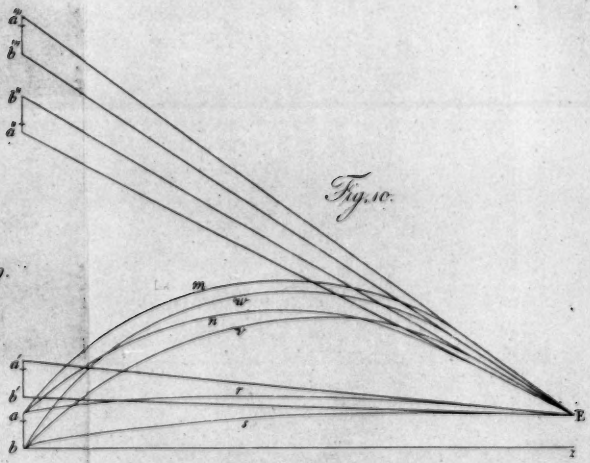

Fig. 10.

a n E, *b v* E pass, but should operate in the tract through which *a m* E, *b w* E pass, an erect image would be visible, but there would be no inverted image; and, should it operate in the latter case, but not in the former, there would be only an inverted image.

As the phænomena are very curious, and extraordinary in their nature, and have not, that I know of, been before observed, I have thought proper to lay a description of them, with all the attending circumstances, before the Royal Society. They appear to be of considerable importance; as they lead us to a knowledge of those changes to which the lower parts of the atmosphere are sometimes subject. If, when these phænomena appear, a vessel, furnished with a barometer, thermometer, and hygrometer, below, and also at the top of the mast, were sent out to pass below the horizon and return again, and an observer at land, having like instruments, were to note, at certain intervals, the situation and figure of the images, it might throw further light upon this subject, and lead to useful discoveries respecting the state of the atmosphere, from a conjunction of the causes which affect these instruments.

III. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1797. *With some Remarks on the Recovery of injured Trees. By* Thomas Barker, *Esq. Communicated by Mr.* Timothy Lane, *F. R. S.*

Read December 13, 1798.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.	
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inches.
Jan.	Morn.	29,99	29,01	29,58	46	33	39	48	26	37	1,319
	Aftern.				48	30	40	51	29	39	
Feb.	Morn.	30,07	28,84	50	47	37	42	46½	25	34	0,076
	Aftern.				48	38½	43½	52½	32	42½	
Mar.	Morn.	29,90	29,03	47	46	38	41½	46	29½	35½	0,908
	Aftern.				47	39	43	53	30½	44½	
Apr.	Morn.	29,70	28,63	31	51½	42½	46	52	36	42½	2,882
	Aftern.				52½	43½	47	59	37	49	
May	Morn.	29,86	28,44	38	67	44	53	64	38	50½	2,528
	Aftern.				69	44½	54½	76	41½	59½	
June	Morn.	29,85	28,93	41	60	52	57	63	39½	52½	4,221
	Aftern.				62½	53½	58	76	43½	63	
July	Morn.	29,80	29,05	49	70½	57	63	66	54	61	3,075
	Aftern.				72½	59	65½	83	60	72½	
Aug.	Morn.	29,72	29,05	35	64	58	60½	64	49	58	2,415
	Aftern.				65½	59	62	76	62	68	
Sep.	Morn.	29,67	28,67	28	62	51½	56	58	43	50	4,792
	Aftern.				64	54	57	70½	51	60	
Oct.	Morn.	29,83	28,57	37	55½	41½	49½	55	31½	44	1,143
	Aftern.				57½	42½	50	63	42½	52	
Nov.	Morn.	29,98	28,50	43	49½	34	43½	51½	24½	38½	1,620
	Aftern.				49½	34½	44	53	30	44	
Dec.	Morn.	29,93	28,60	27	49½	36	42	53	29	38½	2,875
	Aftern.				49½	36½	42½	53	32	42	
											27,854

After a few misty days, the beginning of the year was frosty, winterly, and severe, yet not so hard as before; with frequent but little rains. The end of January, all February, and beginning of March, there was hardly any rain at all; a calm, dry, and pleasant time, but not forwarding, for there was frequent frost, scarcely going off in the shade all day, yet the noontide sun was very warm. The greater part of March was dry, with frequent frosty mornings, and very backward, though calm; but the last ten days were showery, pleasant, and forwarding: this was the beginning of a very wet half year. It was a blasting spring; and some fruits were cut off, after they seemed set, and considerably grown; yet some orchards escaped, and the frequent wet made strawberries plentiful. The season continued cold, wet, and backward, in general, but the last week in April was warmer and more forwarding, yet still wet; then it turned cold again, but there were a few hot dry days toward the end of May. It was wet again at the end of that month; very much so, almost all June, and the beginning of July, with several unseasonable floods, and cold at times; but the rest of July was fairer for some time, and was the best and hottest part of the summer, till a very great thunder storm on the 30th. There was a great crop of hay, but bad weather for getting it up, and in the low meadows it was much flooded. The harvest was good, as to quantity, but could not be all well got, on account of the bad season, yet there was much less complaint of grown corn than might have been expected; perhaps, what was not well got, might be used, not so much for bread, as for starch, powder, and distilling; for there was a vast difference in the price of good and bad wheat, some sorts being almost twice as much as others. Most sorts of fruit were scarce; but peaches and nectarines

were more plentiful, and finer, than either apricots before, or apples after them. I think the pears and apples were more defective in size than in number, for they were remarkably small. The harvest was all showery; but in August the weather was more moderate and tolerable than in September, when there were several great rains and storms.

The autumn, beginning with a few of the last days of September, was in general fine, fair, open, and mild, with moderate winds, and when calm, perfectly so; a good wheat seed time, which came up well; but it was wetter again the end of November, and in December, which kept the ground (already much soaked by the very wet summer) from drying so much as it otherwise might have done. It continued, however, an open winter, not much frost, and that not severe.

On the Recovery of injured Trees.

ABOUT the year 1788 or 1789, a Lucombe oak was planted, the top of which might be about six feet high, but was broken off in coming down. In spring, the tree put out at the two highest buds, but much better at some lower ones. In 1791, the two highest buds again put out, yet, as before, very indifferently; however, a lower bud, about five feet high, put out a strong shoot, about fifteen inches long; but, as side branches are apt to do, it did not grow upright, but slanting. In winter, I fixed that strong shoot upright, by tying it to another shoot, which came out of the opposite side of the tree; and, in 1792, it made a very strong, straight, and upright shoot, three feet nine inches long, and as thick as a moderate finger, and has continued thriving ever since. The tree is now about eighteen

feet high, and eleven inches in girth at the bottom; it has been pretty well cleared of boughs about half way up, and may perhaps, by degrees, be cleared several feet higher.

In the winter of 1789, an ash tree was cut down, which, falling against a young oak, as thick as my leg, beat it down. I had the oak cut close to the ground, and in 1790 it put out a number of shoots, which grew that year, and 1791. In 1792, I chose out the best shoot, trained it up as straight as I could, and beat down the rest of the shoots to the ground, under the hedge, to weaken them, and encourage the best shoot, which I intend to be the tree. It has since grown strong, is pretty straight, has been pruned, and may, I believe, by degrees, be cleared to a good height, for the leading bud is strong and upright. It is now (in August, 1798) about 14½ feet high, and about 6½ inches in girth at the bottom.

IV. *Some Additions to a Paper, read in 1790, on the Subject of a Child with a double Head.* By Everard Home, *Esq. F. R. S.*

Read December 13, 1798.

In the year 1790, I laid before this learned Society, an account of a child with a double head, illustrated by drawings, which is honoured with a place in the Philosophical Transactions, Vol. LXXX.

Since that time, Mr. Dent, the gentleman who sent over from India the double skull, which was shewn at the meeting when the Paper was read, has returned to England. Among his drawings there are two portraits of the double head, taken by Mr. Devis, an artist of considerable merit, who was upon a visit at Mr. Dent's house, in Bengal, when the child was brought there alive, to be shewn as a curiosity. These drawings give a more faithful representation of the appearance of the double head, than the engravings annexed to the former Paper, and at the same time exhibit a striking likeness of the child's features.

Mr. Dent has given permission to have them copied; and, from a desire to render as complete as possible, the account and representation of so curious and rare an instance of deviation from the natural structure in the human body, they are now offered to the Society, (see Tab. II. and III.) with some observations made by Mr. Dent, as a supplement to the former Paper.

Mr. DENT's observations, in addition to those already in the possession of the Society, are the following.

The child was a male. Its father was a farmer at Mundul Gaut, in the province of Bardwan, who told Mr. DENT, that it was more than four years old at the time of its death.*

The mother, who was thirty years of age, had three children, all naturally formed; and her fourth child was the subject of the present Paper. Mr. DENT endeavoured to discover whether any imaginary cause had been assigned by the parents, for the unnatural formation of the child; but the mother declared, that no circumstance whatever, of an uncommon nature, had occurred: she had no fright, met with no accident, and went through the period of her pregnancy exactly in the same way as she had done with her other children.

The body of the child was uncommonly thin, appearing emaciated from want of due nourishment.

The neck of the superior head was about four inches long; and the upper part of it terminated in a hard, round, gristly tumour, nearly four inches in diameter.

The front teeth had cut the gums, in the upper and under jaws of both heads.

When the child cried, the features of the superior head were not always affected; and, when it smiled, the features of the superior head did not sympathize in that action.

In preparing the skull, which unpleasant operation Mr. DENT was obliged, from the prejudices of his servants, to superintend, he found that the dura mater belonging to each brain, was continued across, at the part where the two skulls joined, so that

* In the former Account, the child is said to have been about two years old at that time.

each brain was invested, in the usual way, by its own proper coverings; but the dura mater which covered the cerebrum of the upper brain, adhered firmly to the dura mater of the lower brain: the two brains were therefore separate and distinct, having a complete partition between them, formed by an union of the dura matres.

When the contents of the double skull were taken out, and this union of the dura matres more particularly examined, a number of large arteries and veins were seen passing through it, making a free communication between the blood vessels of the two brains. This is a fact of considerable importance, as it explains the mode in which the upper brain received its nourishment.

Before these observations were communicated by Mr. DENT, it was natural to suppose that the two brains had been united into one mass; as it was difficult to imagine in what way the upper brain could be supplied with blood.

V. *Observations on the Manners, Habits, and Natural History, of the Elephant.* By John Corse, *Esq.* *Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read January 24, 1799.

Since the remotest ages, the elephant, on account of his size, his sagacity, and his wonderful docility, has attracted the notice, and excited the admiration, of philosophers and naturalists, both ancient and modern; and few travellers into Asia, or Africa, have omitted giving some account of him.

A residence, however, of more than ten years, in Tiperah, a province of Bengal, situated at the eastern extremity of the British dominions in Asia, where herds of elephants are taken every season, afforded me frequent opportunities of observing, not only the methods of taking them, but also the habits and manners of this noble animal.

From the year 1792 till 1797, the elephant hunters were entirely under my direction; so that I had it in my power to institute such experiments as I thought likely to discover any particulars, not formerly known, in the natural history of the elephant. Soon after my arrival at Tiperah, while informing myself of the methods of taking wild elephants, I had occasion to observe, that many errors, relative to the habits and manners of that useful animal, had been stated in the writings of European authors, and countenanced by some of the most approved writers.

The elephant has been declared to possess the sentiment of

modesty in a high degree; and, by some, his sagacity was supposed to excite feelings for the loss of liberty, so acute, as to cause him to refuse to propagate his species while in slavery, lest he should entail on his progeny a fate similar to his own; whilst others have asserted, that he lost the power of procreation in the domestic state.

So circumstanced, I was desirous of taking advantage of my situation, and of making such experiments and observations, as might tend to render more perfect the natural history of this useful animal.

Early in the year 1789, I gave an account of the methods then used for taking and training wild elephants, to the Asiatic Society in Calcutta, which was published in Vol. III. of their Researches; and the following experiments and observations, made since that period, on the natural history of the elephant, will not, I hope, prove unworthy the attention of the Royal Society.

The young of the elephant, at its birth, is about 35 inches high; and, as a knowledge of its progressive growth forms the best criterion by which we can judge of the age of this animal, I shall here note down some observations made on this subject, till the elephant has attained its full size; for, after this period, till signs of old age appear, I do not know any marks by which a tolerable guess can be made of the number of its years, unless we could examine the teeth accurately; and, even then, there would be much uncertainty.

Very erroneous notions have been entertained, with respect to the size of elephants, in different parts of India; for which reason, I have collected such facts as were likely to ascertain their general height. The following observations, of

the gradual increase of growth, were made upon a young elephant of Mr. STEPHEN HARRIS, which was accurately measured from time to time, and upon a female elephant of my own, till I left Tiperah.

Mr. HARRIS's elephant, at its birth, October 16, 1789, was 35 inches high.

	Feet.	Inches.
In one year he grew 11 inches, and was	3	10 high.
In the 2d year 8 - -	4	6
In the 3d year 6 - - -	5	0
In the 4th year 5 - -	5	5
In the 5th year 5 - - -	5	10
In the 6th year $3\frac{1}{2}$ - -	6	$1\frac{1}{2}$
In the 7th year $2\frac{1}{2}$ - -	6	4

Except during his 4th and 5th years, the above measurement shows a gradual decrease in the proportion of growth for every year; and there was no opportunity of tracing the growth of this elephant further than its 7th year.

Another elephant, six feet nine inches high, at the time she came into my possession, was supposed to be fourteen years old; but, as the accuracy of the hunters cannot be depended on, it will be proper to take Mr. HARRIS's elephant, whose age is exactly known, as a standard; and, judging from its annual increase, this will lead us to consider the elephant, at the time I received her, to be only eleven years old; giving a period of four years, for the addition of five inches. I have made a greater allowance of time, on account of this elephant being a female, and Mr. HARRIS's a male, which there is much reason to believe grows faster.

During the next five years, before she was covered, she grew

only six inches; but, what is extremely curious, while pregnant, she grew, in twenty-one months, five inches; and, in the following seventeen months, though again pregnant, she grew only half an inch; at which time, she was sent from Comillah, as I was then preparing to leave India.

At this time, she was about 19 years old, and had perhaps attained her full growth. Her young one was then (Nov. 1796) not twenty months old; yet he was four feet five inches and a half high, having grown eighteen inches since his birth; which is the greatest progressive growth, in the elephant, that I have known.

These observations, when applied to the general growth of elephants, are to be taken with some allowance; since, during the state of the first pregnancy, there is so great an irregularity in the growth of female elephants, as alone occasions considerable difficulty, even supposing the progressive growth nearly equal in the species. It is probable, however, that this is not by any means equal: for, as elephants vary greatly in size, and as males are generally much taller than females, we must conclude they either grow faster, or are longer in attaining their full growth.* But it may be safely asserted, that elephants, like most quadrupeds, propagate their species before they have acquired their full growth. Many females have been known, when taken while pregnant, to have grown several inches higher before delivery; and, as it has been stated, that the female elephant on which my observations were made, could not exceed 16 years when she received the male, it is probable the wild female elephants are in heat before that period.

* A male elephant, belonging to the Cudwah Rajah till he was above twenty years of age, continued to increase in height, and was supposed not to have attained his full size, when I left Tiperah: he was then about twenty-two years old.

If, from the above data, it may be allowed to form a probable conjecture, elephants attain their full size between eighteen and twenty-four years of age. The height of the elephant, I believe, has been generally much exaggerated. In India, the height of females is, in general, from seven to eight feet; and that of males, from eight to ten feet, measured at the shoulder.

I have never heard but of one elephant, on good authority, that much exceeded ten feet: this was a male, belonging to ASOPH UL DOWLAH, the late vizier of Oude. His dimensions, as obligingly communicated to me by Mr. CHERRY, then Resident at Lucknow, were as follow.

He was measured on the 18th of June, 1796.

	Feet.	Inches.
From foot to foot, over the shoulder - -	22	10½
From the top of the shoulder, perpendicular height	10	6
From the top of the head, when set up, as he ought to march in state - - - -	12	2
From the front of the face to the insertion of the tail - - - - -	15	11

Capt. SANDYS, of the Bengal establishment, obligingly shewed me a list of about 150 elephants, of which he had the management during the late war with Tippoo Sultaun, in Mysore, and not one of them was ten feet, and only a few males nine feet and a half high. I was very particular in ascertaining the height of the elephants employed at Madras, and with the army under Marquis CORNWALLIS, where there were both Ceylon and Bengal elephants; and I have been assured, that those of Ceylon were neither higher, nor superior, in any respect, to those of Bengal; and some officers assert, that they were considerably inferior, in point of utility.

The Madras elephants have been said to be from seventeen to twenty feet high; but, to show how much the natives of India are inclined to the marvellous, and how liable Europeans themselves are to mistakes, I will relate a circumstance that happened to myself.

Having heard, from several gentlemen who had been at Dacca, that the Nabob there had an elephant about fourteen feet high, I was desirous to measure him; especially as I had seen him often myself, during the year 1785, and then supposed him to be above twelve feet. After being at Tiperah, and having seen many elephants caught, in the years 1786, 1787, and 1788, and finding all of them much inferior in height to what I supposed the Nabob's elephant, I went to Dacca, in 1789, determined to see this huge animal measured. At first, I sent for the driver,* to ask some questions concerning this elephant; he, without hesitation, assured me he was from ten to twelve cubits, that is, from fifteen to eighteen feet high; but added, he could not, without the Nabob's permission, bring me the elephant to be examined. Permission was accordingly asked, and granted: I had him measured exactly, and was rather surprised to find he did not exceed ten feet in height.

The Honourable Company's standard, for serviceable elephants, is seven feet and upwards, measured at the shoulder, in the same manner as horses are. At the middle of the back, they are considerably higher; the curve or arch of which, particularly in young elephants, makes a difference of several inches.

After an elephant has attained his full growth, it is a sure sign of old age when this curve becomes less; and still more

* Or *Mahot*, as he is generally called.

so, when the back is flat, or a little depressed. A partial depression of the spine is, however, no uncommon occurrence, even in very young elephants; and I am convinced it happens from external injury. I have been surprised to see the violence used (in herds of wild elephants just taken) by the large elephants, both male and female, putting the projecting part of the upper jaw, from which the tusks grow out, on the spine of the young ones, and pressing them to the ground, while they roared from pain.

It has been stated, that the sagacity of the elephant is so great, and his memory so retentive, that when once he has received an injury, or been in bondage, and afterwards escapes, it is not possible, by any art, again to entrap him. Great as my partiality is for this noble animal, whose modes of life and general sagacity I have had so many opportunities of observing, yet a regard to truth compels me to mention some facts, which contradict that opinion. The following history of an elephant taken by Mr. LEEKE,* of Longford Hall, Shropshire, contains many interesting particulars on this subject. The elephant was a female, and was taken at first, with a herd of many others, in the year 1765, by Rajah KISHUN MAUNICK,† who, about six months after, gave her to ABDOOR REZAH, a man of some rank and consequence in the district. In 1767, the Rajah sent a force against this ABDOOR REZAH, for some refractory conduct, who, in his retreat to the hills, turned her loose into the

* He was then the Resident of Tiperah, and took some pains to ascertain the facts here mentioned.

† The Rajah is the principal Zemindar in the province of Tiperah, paying the usual revenue for his lands in the low country; but, in the hills he is an independent sovereign, has the power of life and death over his subjects, a mint, and other insignia of sovereignty.

woods, after having used her above two years, as a riding elephant. In January, 1770, she was retaken by the Rajah; but, in April, 1771, she broke loose from her pickets, in a stormy night, and escaped to the hills. On the 25th of Dec. 1782, she was driven by Mr. LEEKE's elephant hunters into a *keddab*;* and, the day following, when Mr. LEEKE went to see the herd that had been secured, this elephant was pointed out to him by the hunters, and particularly by a driver who had had charge of her for some time, and well recollected her. They frequently called to her by name; to which she seemed to pay some attention, by immediately looking towards them, when her name, *Juggut-Peauree*, was repeated; nor did she appear like the wild elephants, which were constantly running about the *keddab* in a rage, but seemed perfectly reconciled to her situation.

From the 25th of December to the 13th of January, (a space of eighteen days,) she never went near enough the outlet (or *roomee*) to be secured; from a recollection, perhaps, of what she had twice before suffered.† Orders, however, had been given, not to permit her to enter the outlet, had she been so inclined, as Mr. LEEKE wished to be present when she was taken out of the *keddab*. On the 13th of January, 1783, Mr. LEEKE went out, when there were only herself, another female, and eight young ones, remaining in the inclosure. After the other female had been secured, by means of the *koomkees*‡ sent

* The inclosure in which elephants are secured. *Vide* Asiatic Researches, Vol. III. Art. "Method of catching Elephants."

† When elephants were secured in the outlet from the *keddab*, they bruised themselves terribly. *Vide* Asiatic Researches, Vol. III.

‡ *Koomkees* are female elephants, trained for the purpose of securing wild elephants, and more particularly those large males which stray from the woods, named *goondahs*. *Vide* Asiatic Researches, Vol. III.

in for that purpose, the hunters were ordered to call *Juggut-Peauree*. She immediately came to the side of the ditch, within the inclosure; on which, some of the drivers were desired to carry in a plantain tree, the leaves of which she not only took from their hands, with her trunk, but opened her mouth, for them to put a leaf into it, which they did, stroking and caressing her, and calling to her by name. Mr. LEEKE, seeing the animal so tame, would not permit the hunters to attempt tying her; but ordered one of the trained elephants to be brought to her, and the driver to take her by the ear, and order her to lie down. At first, she did not like the *koomkee* to go near her, and retired to a distance, seemingly angry; but, when the drivers, who were on foot, called to her, she came immediately, and allowed them to stroke and caress her, as before; and, in a few minutes after, permitted the trained females to be familiar. A driver, from one of these, then fastened a rope round her body, and instantly jumped on her back; which, at the moment, she did not like, but was soon reconciled to it. A small cord was next fastened round her neck, for the driver to put his feet in, who, seating himself on the neck, in the usual manner, drove her about the *keddah*, the same as any of the tame elephants.

After this, he ordered her to lie down, which she instantly did; nor did she rise till she was desired. He fed her from his seat, gave her his stick to hold, which she took with her trunk, and put into her mouth, kept, and then returned it, as she was directed, and as she formerly had been accustomed to do. In short, she was so obedient, that had there been more wild elephants in the *keddah*, to tie, she would have been useful in securing them.

Mr. LEEKE himself then went up, took her by the ear, and bade her lie down; a command she instantly obeyed.

I have known several other instances of elephants being taken a second time; and was myself a witness both of the escape and retaking of one, as related in the following account.

In June, 1787, *Játtra-Mungul*, a male elephant, taken the year before, was travelling, in company with some other elephants, towards Chittigong, laden with a tent and some baggage, for our* accommodation on the journey. Having come upon a tiger's track, which elephants discover readily by the smell, he took fright, and ran off to the woods, in spite of the efforts of his driver. On entering the wood, the driver saved himself, by springing from the elephant, and clinging to the branch of a tree under which he was passing; when the elephant had got rid of his driver, he soon contrived to shake off his load. As soon as he ran away, a trained female was dispatched after him, but could not get up in time to prevent his escape; she, however, brought back his driver, and the load he had thrown off, and we proceeded, without any hope of ever seeing him again.

Eighteen months after this, when a herd of elephants had been taken, and had remained several days in the inclosure, till they were enticed into the outlet, there tied, and led out in the usual manner, one of the drivers, viewing a male elephant very attentively, declared he resembled the one which had run away. This excited the curiosity of every one, to go and look at him; but, when any person came near, the animal struck at him with his trunk, and, in every respect, appeared as wild and outrageous as any of the other elephants.

* Mr. BULLER and myself.

At length, an old hunter, coming up and examining him narrowly, declared he was the very elephant that had made his escape about eighteen months before.

Confident of this, he boldly rode up to him, on a tame elephant, and ordered him to lie down, pulling him by the ear at the same time. The animal seemed quite taken by surprise, and instantly obeyed the word of command, with as much quickness as the ropes, with which he was tied, permitted; uttering, at the same time, a peculiar shrill squeak through his trunk, as he had formerly been known to do; by which he was immediately recognized, by every person who had ever been acquainted with this peculiarity.

Thus we see that this elephant, for the space of eight or ten days, during which he was in the *keddah*, and even while he was tying in the outlet, appeared equally wild and fierce as the boldest elephant then taken; so that he was not even suspected of having been formerly taken, till he was conducted from the outlet. The moment, however, he was addressed in a commanding tone, the recollection of his former obedience seemed to rush upon him at once; and, without any difficulty, he permitted a driver to be seated on his neck, who, in a few days, made him as tractable as ever.

These, and several other instances which have occurred, clearly evince, that elephants have not the sagacity to avoid a snare into which they have, even more than once, fallen.

The general idea, that tame elephants would not breed, has doubtless prevented trials being made, to ascertain whether, under particular circumstances, this supposed reluctance could be got the better of.

I was however convinced, from observation, as well as from

some particular facts, that elephants had their seasons in which they were in heat; I shall, therefore, first mention the circumstances which induced me to attempt breeding from tame elephants, and then relate the success of the experiments instituted for this purpose.

The circumstances to which I allude, happened in January, 1790, at a *keddah* near to Comillah, the capital of Tiperah.

Messrs. HENRY BULLER and GEORGE DOWDESWELL, of Chittigong, being then on a visit at Comillah, accompanied me and several others, to see a herd of elephants which had been lately taken. Our visitors then proposed a trial being made, of tying the wild elephants immediately, in the *keddah*, in the manner practised at Chittigong, instead of waiting till they were enticed, one after another, into the narrow outlet, there to be secured, and led out in the usual manner.*

This mode they recommended so earnestly, from a conviction of its superior utility,† that Mr. JOHN BULLER, to whom

* *Vide* Asiatic Researches, Vol. III. article, "Method of catching wild Elephants;" where this process is particularly described.

† Though fully convinced of this, I could not bring the hunters to adopt the Chittigong method, till the year 1794. After this, during the last three years I remained at Tiperah, I did not lose one elephant in twenty; whereas, by the former method, of tying them in the *roomes*, near one-third of those taken died in less than a year, in consequence of the hurts they received from their violent efforts to get free, before they could be properly secured. The natives of Tiperah, and indeed of most parts of India, are extremely attached to old customs; and it was with the utmost difficulty I prevailed on the hunters to deviate from the practice of their ancestors, though the method recommended was followed at Silhet, as well as at Chittigong. The method was, simply to surround a herd, in the first convenient place, with a ditch and palisade; and, when this was finished, to send in the *koomkees*, and proper persons to tie the wild elephants on the spot, and then conduct them, one by one, through an opening in the palisade, from the *keddah*, as soon as they were tied.

the *keddah* belonged, assented to the trial being made, and gave orders for the trained females, and proper assistants, to go directly within the inclosure. Having but few trained females present, it was judged advisable to send in a fine male elephant, taken many years before, and thoroughly broke in, to assist them, as well as to keep the herd in awe. He had no sooner entered the inclosure, and been brought near the herd, than, discovering one of the females to be in heat, impelled by desire, and eager to cover her, he dashed through the herd, regardless of the orders and severe discipline of the driver, and had nearly accomplished his purpose. The driver, being alarmed for his own safety, exerted in vain all his strength, to turn him, and bring him from among the wild elephants; but the drivers of the trained females, coming speedily to his assistance, soon surrounded this furious animal, and separated him from the herd. In resentment, however, of his disappointment, he attacked a small *koomkee*, with such violence as completely overturned her and her rider; and, had he not been of a particular species, called *mucknab*, which have only small tusks, he most probably would have transfixed, and killed her on the spot: fortunately, neither she nor her driver received any considerable hurt. This accident prevented the trial being then made, to tie the wild elephants in the manner proposed.

Reflecting on the disobedience shown by an elephant remarkably docile, and which had been domesticated for many years, when his passions were excited, and recollecting also, that a wild elephant had covered a female, in February, 1778, before many spectators, just after the herd had been secured in the inclosure, I was assured in my own mind, that it was not from any sense of modesty, either wild or tame elephants did

not gratify their passions in public; but no opportunity offered of prosecuting this inquiry, till 1792. Having then taken upon myself the management of the elephant hunters, a very fine male was caught in November: he was both young and handsome, and also of a most docile disposition; I therefore promised his driver a considerable gratuity, if he would get him into high order, so that I might have an opportunity of bringing his procreative powers to trial, with a tame female.

In the month of March, 1793, the driver of a favourite female elephant informed me, that she had then signs of being in heat; and that, if the male and she were kept together, and highly fed, an intimacy would probably soon take place. They were therefore, shortly after this, brought near to Comillah, where a spacious shed was erected for their accommodation.

In the day, they went out together, to feed; they also brought home a load of such succulent food as their drivers and attendants could collect. After their return, they stood together, slept* near each other, and every opportunity was granted them to form a mutual attachment. In the evening, they had each from ten to twelve pounds of rice soaked in water, to which a little salt was added; and, from the middle of May till the latter end of June, some warm stimulants, such as onions, garlic, turmeric, and ginger, were added to their usual allowance of rice. Long before this, however, a partiality had taken place, as was evident from their mutual endearments, and caressing each

* It is always a good sign, when an elephant lies down to sleep, within a few months after he is taken; as it shews him to be of a good temper, not suspicious, but reconciled to his fate. Elephants, particularly *goondahs*, have been known to stand twelve months at their pickets, without lying down to sleep; though they sometimes take a short nap standing.

other with their trunks; and this without ceremony, before a number of other elephants, as well as their attendants.

Near the end of June, I was satisfied the male would not, even to regain his freedom, quit the object of his regard; I therefore ordered the keepers to picket the female, by one of her fore-legs only, in the house where they stood, but to leave the male at full liberty. Fearful, however, of hurting their supposed delicacy, and thinking the nearness and sight of the attendants might possibly give umbrage to their modesty, I desired them to remain quiet in a little hut, erected on the outside of the building appropriated to the elephants, where they could see equally well as if nearer.

On the evening of the 28th of June, 1793, the male was let loose from his pickets; and, soon after, he covered the female without any difficulty, although before this she never could have received the male, being taken when very young, about five years and a half prior to this period. The male was then led quietly to his stall; but, early on the morning of the 29th, he became so troublesome, that the drivers, in order, as they said, to quiet him, but partly, I suspect, to indulge their own curiosity, permitted him to cover her a second time; which he readily did, before the usual attendants, as well as a number of other spectators. After this, the driver brought me a particular account of the whole process. Though much pleased with the success of the experiment, yet I was rather chagrined he had not given me notice, that I might have been myself an eye witness; and therefore told him, he should not receive the promised reward, till I had satisfied myself of the fact.

About two in the afternoon of the same day, I was desired to repair to the place where the elephants stood, as the male had

been trying to get nearer the female. On this, I proceeded to the spot, with my friend Captain ROBERT BURKE GREGORY: when we arrived, I ordered the male to be freed from his shackles; and, after some toying, and a few mutual caresses, we had the satisfaction to see him cover the female.

When the male mounted, he placed one of his fore-legs on each side of her spine, with his feet turned to, and pressing against, her shoulders, and his trunk over her forehead; supporting himself firmly in this situation, during coition, which he continued nearly the same time, and in the same manner, as a horse with a mare.

The female remained perfectly still, during the coitus. When the male had finished, he stood quietly by her side, while she caressed him with her trunk; and, as they then appeared well pleased, and gentle as usual, I went up and patted them both, as I had formerly been accustomed to do, without the smallest apprehension. In the evening, they were brought home to be fed; and, though only a few hours had elapsed since his last embrace, the male seemed inclined to make another attempt; to which I would have consented, to gratify a crowd of people then present, had I not now learned, that he had covered the female in the open plain, about ten in the morning, when going out for food, in spite of the exertions of the drivers and attendants; at least so they alleged, in excuse for having permitted it, contrary to my orders. As he had already covered four times in about sixteen hours, I was afraid a further indulgence might be prejudicial, and therefore would not permit it; especially as Mr. IMHOFF, to whom he then belonged, was absent. That gentleman, however, returned two days after; but, when the two elephants were brought together, in order that Mr. IMHOFF's curiosity might be indulged with so novel a sight, the

female, being no longer in heat, was so uncivil as to give the male a kick in the face, when he was using what she then thought improper liberties; nor did she afterwards permit him to cover her, though, when standing together, they mutually indulged in a few caresses.

During the time they were kept together, the male never showed signs of his passions being excited, by any exudation from the ducts of the glands near his temples; which is generally considered as the sign of a male elephant being peculiarly ready for the female. This, however, I am inclined to believe is a vulgar error; as not one of the male elephants I have seen cover, in a domestic state, nor any of the males which were caught singly, or rather entrapped, by their desire to have connexion with the tame females, had, at those times, the smallest appearance of such an exudation. Had this happened, in any one instance, during my residence in Tiperah, I think I must have known it; for, when this exudation takes place, the elephant has a dull heavy look, and it is dangerous for strangers to go near him. I have seen elephants in this situation, after they had been many years caught; but, though they were then said to have their passions excited, I have never known one to cover during the continuance of this exudation: nor have elephants, so far as I have been able to observe, any particular seasons of love, like horses and cattle. Of five instances of elephants covered at Tiperah, one received the male in February, another in April, a third in June, a fourth in September, and the fifth in October. Besides these, an attempt was made by a tame male, to cover, in the month of January, a wild female, then in heat.* When the female is in

* Many pregnant females are taken every year at Tiperah, and produce young ones in the different months: this clearly shows, that there are no particular seasons during which the females are in heat.

heat, the parts of generation show it, by an unusual fulness of the labia; and, if she is placed near a male, she endeavours, by caresses, to excite his desires.*

After the female had been covered by the male, as has been just related, there being then no other female ready, he was placed with an elephant which had had a young one about four years before this, and some months ago was reported to have been in heat. It was thought, after some trial, that she was likely to permit him to cover, as she caressed him occasionally, and roused his passions; but she would not allow him to gratify his desire.

The drivers, tired of this coyness, and stimulated perhaps by the hopes of another gratuity, were so brutal as to tie her, and let the male make an attempt upon her, while tied. His attempt, however, was to no purpose; though he continued his efforts till he appeared to be quite exhausted. This being told me, I severely reprimanded the people; and ordered the female to be left at full liberty to reject or receive the male, as she might think proper.

Here, however, was positive proof, that the male would have effected his purpose by force, when he found he could not obtain it any other way. He remained at Comillah till October, 1793, without my being able to procure a female that was in heat; he was then sent to Calcutta.

I now became extremely solicitous about the health of the

* It may be proper to observe, that the penis of a full grown elephant is from two feet four to two feet six inches in length, and from fourteen to sixteen inches in circumference. I caused the penis of two males to be measured, after their passions were excited, in order to ascertain the real size. On some occasions, I have seen the penis absolutely touch the ground, when the elephant has been walking; but it must be recollected, that the hind legs of an elephant are very short, in proportion to his size.

female which was covered in June; and gave particular directions not to overheat her, but merely to give her as much food and exercise as were likely to keep her in the best condition, as she was now known to be pregnant. In three months after she was covered, she became fuller, her flesh felt softer, and her breasts began to swell. These marks of her being with young, were so evident to the driver, that he mentioned them of his own accord; which convinced me, that an elephant, three months after conception, may be known by the keepers to be pregnant.

She had always been a favourite, from having been the gift of my worthy and respected friend Mr. JOHN BULLER,* as well as from her gentle and docile disposition; and I now had hopes of her going her full time.

She was seven feet three inches high, when covered; but, after this, increased so fast, not in bulk only, but also in height, as to exceed seven feet eight inches, before she brought forth. On the 16th of March, 1795, she produced a fine male; just twenty months and eighteen days after she was first covered.

The young one was thirty-five inches and a half high; and had every appearance of having arrived at its full time, being the largest I had known produced in Tiperah.

We have many young produced every year, by the females which are taken while breeding, and these seldom exceed thirty-four inches; this, however, may be owing to the weak and reduced state the mothers are brought to, while breaking in.

The young of the elephant, at least all those I have seen, begin to nibble and suck the breast soon after birth; pressing

* Now one of the Members of the Board of Revenue, at Calcutta.

it with the trunk, which, by natural instinct, they know will make the milk flow more readily into the mouth, while sucking. Elephants never lie down to give their young ones suck; and it often happens, when the dam is tall, that she is obliged, for some time, to bend her body towards her young, to enable him to reach the nipple with his mouth; consequently, if ever the trunk was used to lay hold of the nipple, it would be at this period, when he is making laborious efforts to reach it with his mouth, but which he could always easily do with his trunk, if it answered the purpose. In sucking, the young elephant always grasps the nipple (which projects horizontally from the breast) with the side of his mouth.

I have very often observed this; and so sensible are the attendants of it, that, with them, it is a common practice to raise a small mound of earth, about six or eight inches high, for the young one to stand on, and thus save the mother the trouble of bending her body every time she gives suck, which she cannot readily do when tied to her picket.

Tame elephants are never suffered to remain loose; as instances occur of the mother leaving even her young, and escaping into the woods.

Another circumstance deserves notice: if a wild elephant happens to be separated from her young, for only two days, though giving suck, she never afterwards recognizes or acknowledges it. This separation sometimes happened unavoidably, when they were enticed separately into the outlet of the *keddah*. I have been much mortified at such unnatural conduct in the mother; particularly when it was evident the young elephant knew its dam, and, by its plaintive cries and submissive approaches, solicited her assistance.

Here it may be observed, that a female was believed to have gone twenty-one months and three days; being supposed to have been covered on the 13th of January, 1788, some days before she was driven into the inclosure. When I made particular inquiry as to the real time she was taken, the superintendant* of the hunters said it was in January; but the principal hunters† declared she was among the herd taken in February following, and was probably the same elephant Mr. BULLER, Captain HAWKINS, and many others, saw covered on the 9th and 10th of that month. Perhaps, some days prior to this, she might have been covered in the woods, before she was brought into the inclosure; but, as a herd was taken in each of those months, and not kept separate, and two years had nearly elapsed before I thought of making any inquiry, it was impossible for me to determine in which of those months she was really taken; and the only motive I then had for endeavouring to ascertain this point, was to form some probable conjecture as to the period of an elephant's gestation, which has now been ascertained, in the instance before related.

Early in September, 1795, the female that had been covered, and had bred under my own observation, was known to be in heat; this was less than six months after bringing forth. Learning, at the same time, that the Rajah of Cudwah, a principal Zemindar of the province, had a very large male that had been in the family near twenty years, from the time he was about five years old, I sent a messenger, requesting the elephant might be sent to Comillah, which request the Rajah immediately complied with.

To prevent any interruption from the number of spectators,

* The *Dárogah*. † The *Dydars*.

the elephants were put into a small inclosure, on the 17th of September; the female was picketed by one leg, and the young one, to which she was giving suck, was tied to a tree at some distance, fearing, if permitted to run about, he might receive some injury.

After a few caresses from the female, the male at length effected his purpose, and covered her twice the same evening. As the intention of the male elephant's visit was known in the district, and a few days had elapsed since the two elephants were brought together, in order to make them acquainted, the number of spectators was greater than on any other similar occasion.

She was afterwards covered, several times, on the 20th of the same month; the male, in this case, being admitted after an interval of three days, although formerly, in June, 1793, she refused him when only two had elapsed. She again proved with young; and, in November, 1796, being myself in a bad state of health, and under the necessity of returning to Europe, I sent her to Lucknow, together with her young one, at the request of my friend Captain DAVID LUMSDEN: though she was then very big, she was still giving suck.

About a month before that period, I got my friend, Mr. STEPHEN HARRIS, to permit a female of his to be covered; the same which had, in 1793, rejected the attempts of the male to cover her contrary to her inclination. Another messenger was dispatched to Cudwah, for the Rajah's elephant, which was again sent to Comillah. He covered her repeatedly, on the 14th, 15th, and 16th of October, 1796, before many Europeans, as well as natives; and, the last time he covered her, it was evidently contrary to her inclination; so that, in fact, he used

force to effect his purpose, and held her so firmly, that the marks of the nails of his fore-feet were deeply imprinted on her shoulders.

Having mentioned a sufficient number of instances, to prove the ability, as well as the inclination of the elephant, to propagate his species in a domestic state, and that without any signs of modesty, and having ascertained the period of gestation to be twenty months and eighteen days, it may be necessary to observe, that it is a difficult matter to bring a male, which has been taken about the prime of life, into good condition to act as a stallion; for, being naturally bolder, and of a more ungovernable disposition, than the female, he is not in general easily tamed, till reduced very low; and it requires considerable time, as well as much expence and attention, before he can be brought into such high order as is requisite. He must also be of a gentle temper, and disposed to put confidence in his keeper; for he will not readily have connexion with a female, whilst under the influence of fear or distrust. Of this I have seen many instances; nor do I recollect one male elephant in ten, which had been taken after having attained his full growth, much disposed to have connexion with a female. This is a most convincing proof, that those males which are taken early in life, and have been domesticated for many years, more readily procreate their species than elephants taken at a later period. In their wild state, however, they show no reluctance; for, besides all the males that are entrapped, from their desire to have connexion with the trained females which, though not in heat, are carried out to seduce them, several instances have occurred, of wild elephants covering, immediately after being taken, in the *keddah*.

On the 3d of April, 1795, a very fine male elephant covered a female twice, in the midst of the herd, and before all the hunters. On the 4th, I saw him attempting to cover a third time, when he was suddenly disturbed, by the noise the hunters made to drive away some of the herd which had come too near the palisade. In consequence of this interruption, he threw down first one and then another small elephant, and gored them terribly with his tusks, though they came between him and the female only for their protection: he had, before this, killed four, and wounded many others. When the poor animals were thrown down, conscious of their impending fate, they roared most piteously; but, notwithstanding their prostrate situation, and submissive cries, he unfeelingly and deliberately drove his tusks through, and transfixed them to the ground; yet none of the large elephants, not even the dams of the sufferers, came near to relieve them, or seemed to be sensibly affected. This savage animal had been then confined four days in the inclosure, along with the herd, upon a very scanty allowance of food, and could have but very little hope of escaping; yet here his passions were stronger than his fears. It was on account of this savage disposition, the hunters had asked permission to shoot him, before I had either seen him or the herd, and thence judged he was a *goondab*,* that had lately

* From this instance, as well as many concurring circumstances, I am convinced that these *goondabs* generally leave the herd of their own accord, and join it when they think proper, or are induced to it from a female being in heat; yet it has been supposed, that they are driven from the herd, at an early period of life, by their seniors. This appears improbable, as it is not often that very large males are taken with a herd of elephants; for, depending on their own strength, they stray singly, or in small parties, from the woods into the plains, and even to the villages; and it is in these excursions they are taken, by means of the trained females. As these *goondabs*

joined. Having never before known any elephant killed wilfully, in the *keddah*, by the larger males, and having no idea that he would commit such terrible havock, I unluckily refused to grant their request, being desirous to save so stately an elephant. When the palisade was finished, I got him tied, and led out; but, not brooking restraint, he languished about forty days, after he was secured, and then died.

In the course of this narrative, I have, in general, related only such particulars concerning the elephant as came within my own knowledge, and which were either not known, or not published. To enter into a particular history of the elephant was not my intention; and, although the procreation of tame elephants has been proved, yet the expence incurred by breeding them, may deter others from making attempts of this kind. But it opens a field of curious inquiry to the naturalist; and, now that the facility with which it may be done is ascertained, it suggests itself as a mode by which the breed of elephants may be improved, in size, strength, and activity. In this way, any expence which might be incurred, would more than repay itself, in the future benefits to be derived from a superior breed of elephants.

are much larger, and stronger, than the males generally taken with the herd, it is not probable they would submit to be driven from it, unless at an early period. I have seldom seen, in a herd of elephants, a male so large as may be commonly met with among two or three *goondahs*; but, if these last were driven from the herd when young, the very reverse would be observed.

VI. *On the Decomposition of the Acid of Borax or Sedative Salt.* By Lawrence de Crell, *M. D. F. R. S. Lond. and Edinb. and M. R. I. A. Translated from the German.*

Read February 7, 1799.

THE salt called Borax, so useful in various manufactures and arts, and hitherto imported only from Thibet and Persia, or, in small quantities, from Tranquebar,* has ever excited the attention of natural philosophers. This attention was principally directed to the acid (called sedative salt) contained in it; its other component part, the alkaline salt, (soda or natron,) being better known, and found in many other natural productions, either alone, or in conjunction with other acids. The acid abovementioned has hitherto been discovered only by HÖFER, in the lagone of Castelnuovo; by MARTINOVICH, in the petroleum of Gallicia,† mixed with alkaline earth; and by Mr. WESTRUMB, near Luneburg. The scarcity of this acid, and its being found only in the substances and situations abovementioned, occasioned a supposition, in the minds of those who minutely observe and examine the course of nature, that it is not a simple substance, but is formed afresh from a variety of other substances, previously decomposed, by a singular coincidence of operative causes; and, consequently, that it belongs to compounds.

* DEMACHY's *Laborant im Grossen*, Part 2, page 89.

† CRELL's *Annalen*, 1791. T. I. p. 162.

Numerous have been the experiments made by chemists, who supposed they had formed this salt by composition: some described experiments, which they declared to have succeeded with them, though they always failed, when attempted by others;* from which, LEONHARDI concludes, that nothing more can be expected from any similar attempts to produce sedative salt.†

I was surprised that these chemists had never (so far as I knew) examined the subject by the way of analysis, and endeavoured to decompose the sedative salt already formed by nature. Indeed, no great hopes of success could be entertained, as daily experience shows, that though this salt be kept fluid, in the hottest fire, for many hours together, till it becomes a vitrified substance, yet, when it is afterwards dissolved in distilled water, the solution is complete, without any residuum, and it then shoots into crystals of precisely the same salt as before. Notwithstanding all this, when I reflected, that borax is generated only in certain climates of the East, and that its acid is found only in particular substances and situations, as has been already mentioned, I could not but suppose the latter to be the produce of a new formation. This being premised, I considered maturely in what manner the decomposition of this new and extraordinary compound might be attempted. Admitting the composition to be formed by the coalition of a number of different substances, it seemed not improbable but that an acid, penetrating into and dissolving the whole mass, would rather

* See FUCHS *Geschichte des Boraxis.*

† MACQUER's Dictionary, translated by LEONHARDI, 2d edit. Vol. V. p. 588.

associate with some than with others of its various component parts, and thus produce a separation or change of the latter. Besides, as the sedative salt, strong as its operation is (in a high degree of heat) upon almost all neutral salts, has but a faint taste of acid, it might be supposed, that its acid is contained within some unknown species of earth, intimately combined; or within some sort of inflammable matter; or, according to a phrase used in the new system, there might be a deficiency of acid matter; that, therefore, some more powerful acid would probably separate and dissolve the earthy particles, destroy or change the inflammable matter, or impart the acid it might be supposed to want.

My choice, among the different acids, was fixed upon that particular one which, though not always quick in its operation, never fails to penetrate deep into all soluble substances, is nearly related to all inflammable bodies, and possesses an abundance of acid matter; I mean the oxygenated muriatic acid, prepared with manganese. In the application of this menstruum, I resolved to follow the practice established by the constant experience of both ancient and modern chemists; which has taught us, that difficult decompositions of parts closely united, are more easily effected by a gentle, long continued, digestive heat, and repeated distillation of the same menstruum, than by a heat which is more violent, and operates more quickly.

I first made some preliminary experiments, in order to judge what probability there might be of success.

Experiment 1. I poured an ounce and a half of the above-mentioned acid upon two drams of sedative salt, in a retort, to

which I adapted a proper receiver, and then placed the mixture in a gentle digestive heat, of from 140° to 200° of FAHRENHEIT. The fluid was distilled over very slowly, and the salt was dry on the third day. The salt in the retort seemed unchanged; nor had the marine acid lost any thing of its usual smell.

Exp. II. I poured the distilled fluid out of the receiver upon the same salt, and exposed them to the same degree of heat as before. The salt again became dry on the third day, but there was yet no appearance of any change.

Exp. III. I repeated the same process a third time. I now perceived, during the distillatory digestion, several bright yellow spots upon the salt, as it ascended the sides of the retort, resembling well-formed ammoniacal flowers of iron; more of which I discovered, after the entire exhalation of the fluid.

Exp. IV. The above change induced me to repeat the distillation; and I then perceived, not only as many, but a much greater number of bright yellow spots, some of which were even much darker in colour, and approaching to brown. A change had now evidently taken place, which change increased upon every repetition of the process; I therefore judged I might follow this direction with confidence. But, with a view to use the greatest accuracy and precaution in my proceedings and observations, I resolved to begin my work over again.

First, I procured some ounces of sedative salt, which had been obtained from borax by means of vitriolic acid; and then prepared two quarts of the abovementioned oxygenated muriatic acid, by distilling three parts of muriatic acid with one part of the purest manganese, in the usual manner; this I preserved in a cool dark place. Thus, the substances used in the following experiments, were always of the same nature.

Exp. v. I poured three ounces of the oxygenated muriatic acid upon half an ounce of the sedative salt, in a white glass tubulated retort. I used such a retort, that (in frequently pouring back the distilled fluid) I might not have to lute afresh the several vessels, after every distillatory digestion. For the same reason also, I chose a tubulated receiver, the tube of which gradually terminated in a point, in shape of a funnel. This tube passed into a phial, placed in such a manner that all the fluid passing into the receiver dropped immediately into the phial, the joinings of which were closed with bladder. To close the tube of the retort, I did not think it right to use a waxed cork, (though it closes very tight,) because it might be corroded; and also, because the vapours, dropping from the cork, might carry some fat and oily matter back into the retort. For the same reason, I would not use any greasy lute; but closed the joints of the glass stopper (which fitted remarkably close) with a ring of fine sealing-wax, closely pressed upon it, but which could easily be disengaged, after my work was done, while the retort was still warm: and, as I was even afraid of an oily lute about the joints of the receiver, I closed them up with a ring of very fine white clay, which I fitted to them as exactly as possible, by pressure; letting it stand several days, to dry, and then carefully filling up all the cracks. Having made this previous arrangement, and put the abovementioned ingredients together, I suffered them to remain cold for twenty-four hours; at the end of which, the salt was not entirely dissolved, but, upon the application of heat, the whole became a clear fluid.*

* This appeared to me so striking, that I endeavoured to obtain a confirmation of it. I made a similar mixture, in the same proportions, which was not dissolved so long as it remained cold; but was dissolved by heat. When the solution cooled, a

The degree of heat in the sand was from 180° to 240°, by which the fluid evaporated very slowly. During this operation, there ascended, or rather crept up the sides of the retort, a considerable quantity of salt, in very loose flowers, rising pretty high above the fluid, increasing by degrees, and chiefly occupying that half of the retort which received a greater degree of heat than the other; but never the opposite or colder half. In four days, the fire being extinguished towards the evening of the last, the fluid had evaporated, so as to leave the salt apparently dry. After cooling for some time, the bladder upon the phial was moistened by water, and the vessels were separated; the sealing-wax also having been removed, and the stopper taken out, the distilled fluid was poured back, through a glass funnel, upon the salt, without disturbing the lute.

Exp. VI. As soon as the fluid was added, the salt at the bottom began by degrees to dissolve: that on the sides of the retort did the same, after it was heated, but soon began to form again: the solution appeared of a yellowish hue. In general, however, the whole experiment took the same course as in *Exp.* V. and the smell, both of the salt and the fluid, seemed to be unchanged. The only difference was, that the former did not appear like salt, (the crystallization on the sides excepted,) and in single detached crystals, but something like a white, uniform, spongy, and, as it were, earthy mass. The fluid was now again taken from the phial, as in *Exp.* V. and poured back upon the salt.

Exp. VII. VIII. and IX. During the third distillation, bright

small part of the salt (and a larger as the cold increased) precipitated, which was dissolved again, by a fresh application of heat. But, with the degree of heat I employed, no more than one part of salt would dissolve in six parts of the acid.

yellow spots began to appear upon the white flowers; and, after the salt at the bottom had become dry, similar spots appeared upon it, particularly upon the lower surface. The fluid was again, for the fourth time, poured upon the salt, and distilled; when the yellow spots and flowers increased in number. This was also the case in the fifth distillation.

Exp. x. The fluid obtained by the last experiment, which had changed a little in smell, and had acquired a particular scent, almost as if some sebacic acid had combined with the muriatic, was poured upon the salt, as before. The number of yellow spots, which had also become of a darker hue, was considerably increased. The salt had now been exposed, ever since the vth. *Exp.* for thirty-two days, to the digestive distillation; and the intermediate time between each distillation, had been longer or shorter, in proportion to the degree of heat, and to the time of kindling and extinguishing the fire. As I now found, that business of importance would prevent me from continuing my labours for some months, I poured two other ounces of the muriatic acid upon the salt, besides the fluid so often drawn off by distillation, and left the mixture at rest.

Exp. xi. xii. xiii. xiv. When my business was finished, I again undertook the distilling of the mixture, which had been so long digesting in the cold, for the seventh time, and obtained the same results as in *Exp.* x. Nor was there much difference observed in the xiith. xiiith. and xivth. *Experiments.*

Exp. xv. I now poured the fluid obtained by the xivth. *Exp.* upon the salt, (which had acquired more and more yellow spots, brighter in hue,) and then proceeded as before, till the salt became dry; upon which, when the retort was cool, I poured one

ounce and three drams of the muriatic acid, in addition, and allowed the mixture to digest gently for some days.

Exp. XVI. In this twelfth distillation, there appeared a large quantity of flocculent sublimate, looking almost like branches, hanging down, and in many places of a yellow colour; it extended even into the neck of the retort, and almost covered the interior aperture of the tube.

Exp. XVII. The thirteenth distillation produced the same phænomena. Upon the lowermost surface of the mass of salt, many light-brown spots appeared, as soon as the fluid was so much evaporated that no more of it could be seen upon the salt.

From all these circumstances, I now believed the mass of salt, by a digestion of twenty-two days, and seven distillations, from *Exp.* XI. to XVII. (that is, by a digestion of fifty-four days, and thirteen distillations, in the whole,) to be so far decomposed, as to admit of a separation of some of its constituent parts. I therefore supposed I might leave off applying only a digestive warmth, and proceed to a greater degree of heat.

Exp. XVIII. Having poured out the fluid obtained by *Exp.* XVII. and replaced the phial, I increased the degree of heat. By this, the retort became quite obscured, first by fumes, and afterwards by a quantity of white sublimate, attaching itself to all its sides, which, however, had not the appearance of common sedative salt. As I increased the heat, the sublimate grew darker in colour; afterwards became black and frothy; and, at length, ran down the sides of the retort, in different places, like thick oil of hartshorn, the retort being almost wholly blackened by it.

Exp. XIX. While the retort was still warm, I poured into it

the fluid obtained by *Exp.* XVII. having first warmed it a little; when, almost in the same instant, a very agreeable phænomenon took place. Crystals, perfectly white, shot forth suddenly, and all at once, from every part of the black mass covering the sides of the retort. The distillation being continued, these crystals were at length dissolved, and entirely removed. The supernatant fluid was, as usual, almost colourless. When the mass of salt appeared dry, the fire was increased, as in *Exp.* XVIII. and the same appearances as above related took place: first, the sublimate appeared white, then black, frothy, and flowing down the sides.

Exp. XX. I proceeded, as in *Exp.* XIX. to pour back the distilled fluid. Instantly a number of the whitest crystals shot forth from the black ground, forming small groups; but the retort was cracked.

Exp. XXI. I therefore took all the vessels asunder, and shook the retort well, till whatever hung upon its sides was dissolved; then distilled the fluid in another retort, till the mass of salt appeared quite dry. I now put the retort into a crucible, surrounded it with sand, fitted another receiver to it, and placed the crucible in an open fire. First, some sublimate was produced, towards the neck of the retort, (but which vanished as the heat increased,) and then a small portion of fluid, (hardly more than a dram, or a dram and a half,) which appeared to smell a little of the sebacic acid. At the bottom of the retort was a blackish mass, *a*, and likewise some sublimate, *b*, which, by its varied appearance, seemed to be of a two-fold nature.

Exp. XXII. The residuum taken out of the broken retort had a spongy appearance, and swam upon water; it had a blackish colour, and weighed three drams and ten grains.

Being exposed to the air, the blackish colour became lighter, and inclining to grey. When digested in sixteen parts of distilled water, in the usual temperature, for two days and a half, it did not all sink to the bottom; and, after being digested with heat for twenty hours, it was not entirely dissolved: that part which sank, was of a blackish brown. More water was then added, and it was made to boil for two hours; it was afterwards placed upon a paper filter, (the weight of which was previously ascertained,) and edulcorated with boiling distilled water, till at last a proportion of twenty-six parts of water to the substance had been used. After all the fluid, α, had passed through, and the filter, with the residuum, had been dried in a heat of 212°, for an hour and a half, the residuum, β, weighed, exclusive of the filter, nineteen grains.

Exp. XXIII. The fluid, α, obtained by *Exp.* XXI. was suffered to evaporate gradually, and yielded three drams and ten grains of a white transparent salt.

Exp. XXIV. This salt (obtained by *Exp.* XXIII.) was put into a small retort, and exposed, in a crucible filled with sand, to an open fire. It became of a blackish-brown colour, yielded some sublimate, *a*, (about five grains,) a small portion of fluid, *b*, and a blackish-brown residuum, *c*, which grew lighter in colour, on being exposed to the air.

Exp. XXV. The fluid, *b*, (of *Exp.* XXIV.) smelt like marine acid, and precipitated nitrate of lead.

Exp. XXVI. The residuum, *c*, (of *Exp.* XXIV.) by the addition of some water, became whiter, and was dissolved; more water having been added, it was digested with heat, by which the matter was dissolved. The solution being afterwards filtered, I

obtained two drams and four grains of white salt: the residuum upon the filter weighed four grains.

Exp. XXVII. This salt (*Exp.* XXVI.) I again exposed to the fire; when it yielded from twenty to thirty drops of acid liquor, four grains of sublimate, and a residuum, which, being dissolved, yielded one dram and thirty-three grains of salt, and left two grains and a half, ϵ, upon the filter.

The same salt, (obtained by *Exp.* XXVI.) being distilled, became of a brownish-grey colour; and, besides a few drops of fluid, yielded not quite two grains of sublimate. On treating the residuum with water, it yielded sixty-eight grains of salt, and there were not quite two grains left upon the filter.

Exp. XXVIII. On treating these sixty-eight grains of salt in the same manner, they yielded a few drops of fluid, and two grains of sublimate: after filtration, there remained forty-eight grains of salt, and a residuum of hardly one grain and a half.

Exp. XXIX. The same salt, treated in the same manner, yielded a few drops, and a little sublimate; and, after filtration, thirty-five grains of salt, and a residuum of hardly one grain.

Exp. XXX. On treating these thirty-five grains of salt in the same way, they yielded, besides a very small quantity of fluid and of sublimate, twenty-four grains of salt, and about three quarters of a grain of residuum.

As I now discovered that the quantity of salt was continually decreasing, and some coal separating from it, I thought it superfluous to endeavour to decompose the above twenty-four grains any farther.

Exp. XXXI. The residuum, β, of *Exp.* XXII. was light, blackish,

and like coal. I now poured common concentrated muriatic acid upon three grains of it, and digested the mixture, for forty-two hours, in a considerable degree of heat, but no dissolution was apparent. I then added smoking nitrous acid, and digested it for twenty-four hours, till it boiled, without any apparent dissolution. I added some sugar, (about two grains,) but without effect, except that its colour grew yellowish. I now boiled the fluid, till it all evaporated in reddish-yellow vapours: there remained a very black, thick, glutinous mass, smelling like burnt sugar. Having added three ounces of water, the greatest part of the blackish matter rose to the surface, and the water appeared only a little tinged. The fluid part, indeed, became brown by boiling; but, after rest and subsidence, it again grew clear. I filtered it, *a*; then poured two ounces more distilled water upon the residuum, and, after digesting, boiling, and filtering, added the filtered fluid, *b*, to the former, *a*. After this treatment, there remained two grains of residuum, *c*.

Exp. XXXII. Having caused the fluid *a*, *b*, of *Exp.* XXXI. to evaporate, it yielded a salt greyish-yellow mass, which very quickly attracted the moisture of the air. Being again dissolved in water, and saturated with potash, a considerable quantity of whitish earth was precipitated, very much resembling talc.

Exp. XXXIII. The residuum, *c*, of *Exp.* XXXI. which, besides its insolubility and lightness, had much of the external appearance of coal, was now thrown upon melted nitre, and it deflagrated. I placed a second crucible with melted nitre close to it, and having, at the same moment, thrown into one the above-mentioned residuum, and into the other a quantity of common charcoal pulverized, I could not observe the smallest difference in effect. Very little difference was also apparent, as to the

residuum, β. of *Exp.* XXII. *c*, of *Exp.* XXIV. and that of the following experiments.

Exp. XXXIV. To obviate the objection, that sedative salt alone would perhaps deflagrate with melted nitre, I made that experiment also, but in vain. Not the smallest deflagration took place, even when both were melted together for many hours.

Exp. XXXV. Another objection may be made, namely, that in distilling the muriatic acid from manganese, part of the latter had passed over with the acid; and, in the frequent distillations of the sedative salt, had been deposited upon it, and thus deflagrated. But, on throwing fresh pulverized or solid manganese, either such as is usually sold, or quite pure, heated to redness, into melted nitre, not the smallest deflagration took place.

Exp. XXXVI. to L. Instead of the interrupted heat used in the foregoing experiments, I now exposed half an ounce of the salt, with three ounces of the oxygenated muriatic acid, to a continued heat, of between 200° and 300° of FAHRENHEIT. The fluid had nearly evaporated in twenty-four hours. I changed the phial, towards the close of the operation, for another, that the former might be gently heated, and the fluid by that means be poured back, with the greater safety, upon the warm salt, through the tube of the retort. In this manner, during an uninterrupted fire of fourteen days, the acid was fourteen times distilled, and returned upon the salt. On the third day, yellow spots appeared. On the fourth, some particles of oil or fat were discovered, swimming on the surface of the fluid in the phial; which particles, after cooling and emptying the phial, adhered to its sides, so as to obscure its transparency. More or less of these oily particles were discovered in every successive opera-

tion; and the oily matter, adhering to the inside of the glass, increased considerably.

Exp. LI. When the fluid was distilled, the receiver was changed, and the fire increased. A considerable quantity of sublimate was obtained, pretty white in colour, as was likewise the surface of the mass of salt at the bottom of the retort; but, lower down, it was almost of a light ash-grey. After the sublimate ceased to arise, I diminished the fire.

Exp. LII. Upon the mass of the former experiment, I poured the fluid obtained by *Exp.* XLIX. and continued a gentle digestion. I very soon perceived something rising towards the surface, and swimming upon it: after some hours, it appeared to be a thick wrinkled skin, like fat, or a skin of mould, increasing in size, until it covered the whole surface. White spots of sublimate appeared upon it, but it did not sink. It assumed gradually a fine lemon colour; and some yellow matter, though not in large quantity, ascended the sides of the retort. The fluid having been gently distilled, and the receiver changed, I placed the retort in an open fire: on which, more sublimate soon appeared; but, not long after, it all vanished, and the retort lost its transparency. The mass contained in it began to rise, first gently, and then violently, especially in the centre, in large frothy bubbles. The distillation was finished, after obtaining one dram of fluid, and when the frothy bubbling had ceased. The retort being broken, that part where the bubbling had been strongest, was found to be black; the upper surface being covered with a thin greyish matter, under which a solid, compact, and almost vitrified substance appeared. Upon this I poured water, and dissolved it in the usual manner; filtered it,

let it evaporate, and treated it as described above, *Exp.* XXII.—XXX.

Exp. LIII. I obtained a white salt, *a*, and some coal, *b*, (which deflagrated briskly with nitre,) in nearly the same proportions as throughout the series of experiments described from *Exp.* XXII. to XXXIII, which I will not repeat, on account of the little variety observed in them; one of them, however, deserves to be distinguished from the rest.

Exp. LIV. I put six grains of the coal, *b*, (of *Exp.* LIII.) in three drams of common muriatic acid, and digested them for two days, till the acid had gradually evaporated. I then added one dram of the same acid, with one scruple of nitric acid, and, when they had evaporated, boiled the residuum full half an hour in distilled water. By this process, I obtained a red solution; and, having saturated it with mild alkali, a sort of skin rose to the surface, with some small pieces of a fat slippery substance, *a*. A considerable quantity of loose earth, *b*, was also precipitated, of a light brown colour.

Exp. LV. On throwing the floating pieces, *a*, (*Exp.* LIV.) into a solution of caustic alkali, they dissolved; the solution had a reddish-brown colour.

Exp. LVI. With the same solution of caustic alkali, I covered the light brown earth of *Exp.* LIV. As the solution changed its colour to a reddish brown, the earth gradually became perfectly white.

Exp. LVII. To observe the affinity of other acids to the sedative salt, I poured six drams of nitrous acid upon two drams of the salt, with ten drams of the forementioned oxygenated muriatic acid; digested the mixture, and distilled it, in twenty-four

hours, with a gentle heat. Upon the fluid swam a white compact substance, and some small particles of the same kind lay at the bottom, which however rose, on the application of heat, and swam about with the rest.

Exp. LVIII. to LXIII. I poured the whole distillation back upon the salt, and, by means of a digesting heat, again drew off a fluid, which appeared covered with a thin fat skin. I then poured the fluid back, distilled it again, and thus repeated the process three times more. No phænomenon particularly remarkable appeared, except that the thin fat skin grew more inconsiderable, and at last seemed almost to vanish.

Exp. LXIV. The salt separated from the fluid, by the gentle distillation in *Exp.* LXIII. emitted now, by the force of additional heat, dark red vapours, as is usual in strong nitrous acid. When the distillation was at an end, the retort was exposed to an open fire; but, during this operation, no black matter appeared; nor was any coal separated from the mass, upon dissolving it in distilled water.*

Exp. LXV. I now tried the effect of a mixture of four drams of strong vitriolic acid and twelve drams of the muriatic acid, repeating the usual digestion and distillation six times. I will pass over other circumstances, and only mention, that after the sixth distillation of the fluid, a stronger heat, and at length an open fire, was applied; but hardly any fluid was produced, though the fire was so violent, that the whole mass appeared to be melted down into one uniform compact substance.

Exp. LXVI. The vessels having cooled, the mass was of a

* Here the nitrous acid seemed to destroy, and carry off, the inflammable matter, sooner than it could become coal; as it had before occasioned the oily and fat substance to vanish, in the beginning of this experiment.

light milky colour throughout, without the least mixture of brown or black, or any other indication of coal.* Being some time exposed to the air, it became moist, and for a long time attracted much water, which I caused to run off. At last it remained pretty dry; but the mass seemed to have diminished, by at least one-fourth part.

Here I will stop, for the present, in the description of my experiments, which sufficiently tend to prove, in a general way, the decomposition of sedative salt, and to show, that one of its component parts is inflammable matter, which may be converted into coal. I obtained of true coal, (mixed with some earth, *Exp.* XXXII. and LIV.) according to the above-described experiments, (*Exp.* XXII. XXVI.—XXX.) thirty grains and three quarters, in the whole; and by other experiments, often repeated, in general, one grain and a half, more or less. Every other substance liable to be changed into coal, (as gum, tartar, sugar, &c.) suffers this change by a gentle heat, and deflagrates with nitre, in the degree of heat necessary to melt the former. But sedative salt can bear a red heat for many hours, without shewing any signs of becoming coal, of burning, or of deflagration. Astonishing phænomenon! What menstruum preserves it so securely against the assault of force, in a dissolved state, and yet suffers itself to be separated from it by more gentle means? What power exists here, to protect the inflammable particles (which afterwards turn to coal) so effectually against a degree of heat which nothing else can resist? Of what nature is the

* Perhaps here also the remark contained in the former note holds good: yet I am rather of opinion, that the vitriolic acid did not operate with sufficient strength to separate the component parts.

salt obtained in conjunction with the coal? These are all questions which excite great interest, but which are not easily answered. How far I have been successful in resolving them, some subsequent Essays will show; which I shall have the honour to lay before the Royal Society, as soon as I shall have sufficiently repeated the experiments I have already made.

VII. *A Method of finding the Latitude of a Place, by Means of two Altitudes of the Sun and the Time elapsed betwixt the Observations. By the Rev.* W. Lax, *A. M.* Lowndes's *Professor of Astronomy in the University of Cambridge.*

Read January 10, 1799.

I HOPE the following method of determining the latitude, by means of two altitudes of the sun and the time elapsed betwixt the observations, will be found not less convenient for nautical purposes than the rules which are commonly employed. But I would rather recommend it in those cases where rigid accuracy is required, and the astronomer is provided with no better instrument for taking the sun's altitude than a HADLEY's sextant of the most improved construction. The process will be neither difficult nor tedious; and, if the observations are made with proper exactness, I conceive the latitude will generally be obtained within a few seconds of the truth. With these expectations, I have ventured to reduce the method into its present form; and submit it, with the utmost deference, to this learned Society.

In the spherical triangle, whose sides are the complements of the latitude, declination, and altitude, let z represent the angle at the pole, and t its tangent; Z the azimuth, and T its tangent; L the latitude, and λ its cosine, radius being unity; then, if the altitude and declination remain constant, we shall have $\dot{L} = \lambda T \dot{z}$, and, consequently, $\dot{L}$ will vary as $T\dot{z}$, when the increment of λ, com-

pared with λ itself, is inconsiderable. Hence, if the abscisse of the curve *ABCD* (fig. 1.) be always proportional to z, and its ordinate to T, the area *GB* intercepted betwixt any two of these ordinates may represent the increment of the latitude corresponding to the increment of the time *EG*. Let *abcd* (fig. 2.) be another curve, whose abscisse *ae* is always equal to *AE* in the preceding figure, but whose ordinate *eb* is proportional to t, the tangent of the hour-angle; then will the area *gb* vary as *GB*, at small distances from the meridian, and, of course, may represent the increment of the latitude. Now, to prove this, we have only to shew that T and t, when both are small, bear to each other a given ratio. Let S and Σ be the sine and cosine of the azimuth; s and σ the sine and cosine of the angle at the pole; then will $\frac{\dot{T}}{T} = \dot{Z} \cdot \frac{1 + T^2}{T}$, and $\frac{\dot{t}}{t} = \dot{z} \cdot \frac{1 + t^2}{t}$; $\dot{Z} = \frac{\dot{S}}{\Sigma}$, and $\dot{z} = \frac{\dot{s}}{\sigma}$. But, since the complements of the declination and altitude remain constant, whilst the latitude is made to vary, $\dot{S}$ will be to $\dot{s}$ as S to s; and, therefore, we shall have $\frac{\dot{T}}{T} : \frac{\dot{t}}{t} :: \frac{S}{\Sigma} \times \frac{1 + T^2}{T} : \frac{s}{\sigma} \times \frac{1 + t^2}{t}$ $:: 1 + T^2 : 1 + t^2 ::$ the square of the secant of the azimuth : the square of the secant of the hour-angle, which may be considered as a ratio of equality, when the angles are very small. The fluxions, therefore, of the tangents are as the tangents themselves; and, consequently, they must always preserve the same ratio towards each other. Let us now suppose that

FIG. 1.

FIG. 2.

an altitude of the sun is taken at the distance ae from the meridian, but that, in consequence of an error in the assumed latitude, the calculated time is ag; and that, with a lat. differing from the former by one minute, we compute again, and the time is found equal to af; then will the area gc be to gb as one minute to the whole error in latitude. Let another altitude be taken at the distance ae from noon, and let the times computed with the two different latitudes that were employed before be ag and af; then, in this case likewise, the area gc will be to the area gb, as one minute to the error in latitude. Now the latter curve is the "*figura tangentium*," whose quadrature is given by Cotes, in his *Harmonia Mensurarum*, and the expression for which is extremely simple. For the fluxion of the area is $= \frac{ti}{1+t^2}$, and the area itself $=$ log. $\overline{1+t^2}^{\frac{1}{2}} =$ log. secant of the angle at the pole. The difference of the log. secants, or log. cosines, will of course be equal to the area intercepted betwixt the tangents which correspond to them.

Hence a table might easily be constructed with a double argument,—the distance from noon, and the variation in time arising from the different suppositions of latitude,—which might immediately exhibit the logarithm of the area corresponding to any particular base eg supposed to be given. A second table might have for its argument the difference of the logarithms of the area gb and the area gc, (which is likewise conceived to be known) and discover at once, in degrees, minutes, and seconds, the correction to be made in the assumed latitude. This correction, as it appears from a comparison of the signs of $\dot{\mathrm{L}}$ and $\dot{z}$ in the equation $\dot{\mathrm{L}} = \lambda \mathrm{T} \dot{z}$, must be added or subtracted, according as the distance from noon obtained by computation is too great or too

little, when the azimuth is less than 90 degrees; but the contrary, when the azimuth exceeds a right angle. Tables of the above description shall be constructed, if this method be received with approbation; and, in the mean time, it is proposed to subjoin a short specimen which is already completed.

I have presumed that we are able to determine *eg*, the error in time arising from an error in the assumed latitude, at either of the observations; and hence it becomes necessary, before we can avail ourselves of the principles which have been laid down, to point out the manner in which this may be accomplished. The clock gives us the whole interval betwixt the observations (supposed to be made on different sides of the meridian) equal to *ae* + *a*e, and by computation we obtain *ag* + *a*g, and thence we deduce *eg* + eg the whole error in time. Now the area *g b* is equal to gb; and therefore, if we make a rough division of the whole error, without any regard to accuracy, in the inverse ratio of the hour-angles at the two observations, and, entering the first table with these times, mark the area corresponding to each at their respective distances from noon, and increase the one and diminish the other equally, till we get the areas of the same magnitude, this, we may conclude, is the proper value of each.

If the table were constructed to every second of time, we might ascertain these logarithmic areas merely from inspection; but, as it will be advisable to confine it within narrower limits, we shall sometimes find it necessary, as in other tables, to deduce their ultimate value by the rule of three. When we have increased one portion of time and diminished the other, till the difference of their corresponding areas becomes a *minimum*, we must divide this difference betwixt them, in the proportion of

their respective increments in the next interval of time, and subtract or add the part assigned to each, according as it is greater or less than the other. The table, however, might easily be carried to such an extent, that exactness in this division could never be required; but, on the contrary, it would be quite sufficient, when the hour-angles were nearly equal, to add the areas together, and take half the sum for the value of each.

From these principles may be deduced the following practical rule for determining the latitude of a place. When the sun comes within fifteen degrees of the meridian, in the morning, let his altitude be taken, and the time of the observation be accurately marked; and let another altitude be taken after he has passed the meridian, whilst his distance from it is less than fifteen degrees; and let the time of this observation likewise be noted. Then, with the supposed latitude of the place, compute the times corresponding to each of the altitudes in terms of the log. cosine of the hour-angle, and take the difference of the intervals, as shewn by the clock, and determined by calculation, and divide it betwixt the observations in the manner explained above. Compute the log. cosine of the hour-angle a second time, with the greatest altitude and the latitude increased or diminished by a minute, according as it appears, from a comparison of the intervals, to have been too little or too great; and take the difference betwixt this log. cosine and that which resulted from the first operation, when the same altitude was employed. Having thus obtained the two areas gb and gc, we must subtract their logarithms from each other, and with their difference entering the second table we shall find the degrees, minutes, and seconds, by which the assumed latitude is to be increased or diminished.

It will be needless, perhaps, to suggest that, in the higher

latitudes, we may extend the limits above specified a few degrees farther from the meridian, without offering any material violence to the theory, as it has hitherto been explained; and that, on the other hand, when the declination and latitude are nearly equal and of the same denomination, it will be expedient to confine our observations within a much shorter distance from noon. But it will afterwards be demonstrated that, whatever be the magnitude of the hour-angles, or however nearly the latitude and declination may approach towards each other, we can always secure, with very little additional trouble, an exact conclusion.

We may remark that the latitude, determined in this manner, will be nearly equivalent, in point of accuracy, to the mean result of two meridian altitudes. For we know that the increment of latitude : increment of altitude : : radius : cosine of azimuth; and, since the cosine of a small angle differs so little from the radius, this may be considered, within the limits which I have prescribed, as a ratio of equality. If, therefore, one altitude of the sun were taken, and we could ascertain the error in time arising from an error in the assumed latitude without the aid of a second observation, the latitude would be discovered with nearly the same precision as if it had been deduced from the meridian altitude. But, by means of a second observation made on a different side of noon, we obtain a second error in time of the same kind; and this being added to the former, and their sum divided in a just proportion betwixt the two observations, the same effect will be produced, with respect to the accuracy of the result, as if two latitudes had been deduced from meridian altitudes, and a mean betwixt them had been taken.

I might perhaps be allowed to say more; for I am satisfied, from experience, that I can take an altitude of the sun with greater exactness, when he is in any other situation, than when he is upon the meridian. If we could ascertain, within a few seconds, or even within a minute, the time when he attains his greatest altitude, there would then be no reason why an observation should not be made with the same degree of certainty in this, as in other cases; but we are generally obliged to keep our eye stedfastly fixed, for several minutes, upon the two images, and it is well known that, in such circumstances, the best eyes are apt to be deceived. Besides, it is impossible to preserve the contact of the limbs by perpetually moving the index, whilst the sun continues to ascend so very slowly. We are compelled to wait till they are evidently separated, and then, by one turn of the screw, to bring them into contact again, which must necessarily be a source of some inaccuracy. It is for the first of these reasons that, in taking an altitude of the sun, when he is near the meridian, I have found it advisable, not, in the usual manner, to bring the images almost to touch each other, and then to wait till they actually do so, but to bring them at once into contact, with such a degree of velocity as would make them sensibly overlap, or separate, whilst the clock beats a second.

But I consider it as one of the principal advantages of this method, that we can avail ourselves of any number of altitudes, and, of course, approximate as near as we please to a true conclusion with so little additional labour. If there be an equal number of observations made on each side of the meridian, we must combine them together by pairs, according to the preceding instructions, and thus determine the different

logarithmic values of gb. Having then added them all together, and taken a mean betwixt them, we have only to compute a single incremental area gc with any of the altitudes and the lat. varied one minute, and, subtracting its log. from the mean log. value of gb, we shall obtain a very accurate correction of the assumed latitude. But, if there be more observations on one side of the meridian than on the other, when all the pairs have been united, and the areas resulting from them found, we may combine the supernumerary observations on either side with any of those which are made on the opposite side. The properest, however, for this purpose, is the observation which is made at the least distance from the meridian.

I should hope, moreover, the practical astronomer will think it a circumstance of some moment, that the principal part of the work consists in finding the time, an operation which he is obliged so frequently to perform. Any of the three methods which are usually adopted upon this occasion might easily be applied to the tables which have been described; but I will venture to recommend a different rule, which I conceive to be better adapted to our purpose than any of the others, and to which the directions before given had a particular reference.

Let a be the sine of the altitude; γ the cosine of the hour-angle; d the sine, δ the cosine, and r the tangent of declination; l the sine, λ the cosine, and s the tangent of the latitude. Then $\gamma = \frac{a - dl}{\delta\lambda} = \frac{a}{\delta\lambda} - \frac{dl}{\delta\lambda} = \frac{drs}{\delta\lambda rs} - rs = \frac{a}{dl} - 1 \cdot rs$, when radius is unity, but $= \frac{a\,m^2}{dl} - m^2 \cdot \frac{rs}{m^2}$, when radius is $m = \frac{rs}{m^2} \times^{\circ}$. into the square of the tangent of the arc whose secant is $\frac{a\,m^2}{dl}$. Hence we deduce the following rule for deter-

mining the log. cosine of the angle at the pole. From the log. sine of the altitude increased by three times the log. radius, subtract the sum of the log. sines of the latit. and declination; take half of the remainder, and, considering it as the log. secant of an arc, find the log. tangent corresponding; multiply this by 2, and add the log. tangents of the latit. and declin. and reject thrice the log. radius; the sum will be the log. cosine of the angle required. But, when the declin. and latit. are of a different denomination, it is evident that our expression becomes $m^3 + \frac{a\,m^3}{d\,l} \cdot \frac{r\,s}{m^3}$, which is equal to $\frac{r\,s}{m^3} \times s$. into the square of the secant of the arc whose tangent is $\left|\frac{a\,m^3}{d\,l}\right|^{\frac{1}{2}}$. In this case, therefore, having found the log. value of $\frac{a\,m^3}{d\,l}$, and divided it by 2, we must consider the quotient as the log. tangent of an arc, whose log. secant being taken, we are to proceed as in the former case.

The advantages of this rule are obvious. We obtain the angle in terms of the log. cosine; and, consequently, when we have calculated the second time with the new latit. we have only to subtract one result from the other, and we immediately determine the area corresponding to the difference of the times. Besides, in the second computation, fewer of the elements are changed by this rule, than by any of those which are usually employed; and this is a consideration of much importance. But, if we are disposed to adopt the following method of ascertaining the incremental area $g\,c$, this advantage will be found still greater. Let us resume the expression $\gamma = \frac{a - d\,l}{\delta\,\lambda}$, and we shall have $\dot{\gamma} = \frac{1}{\delta} \times \frac{-d\lambda\,\dot{l} - a\dot{\lambda} + d\,\dot{l}\dot{\lambda}}{\lambda\,\lambda'}$ (λ' be-

ing the succeeding value of λ) $= \frac{1}{\dot{l}} \times \frac{\frac{d\lambda^2\dot{\lambda}}{l} - a\dot{\lambda} + d\dot{l}\lambda}{\lambda\lambda'} =$
$\frac{d\lambda^2\dot{\lambda} - al\dot{\lambda} + dl\dot{l}\lambda}{\dot{l}l\lambda\lambda'} = \frac{d\lambda\lambda}{l\lambda'} - \frac{a-dl}{\dot{l}\lambda} \times \frac{\dot{\lambda}}{\lambda'} = \frac{r}{s} - \gamma \cdot \frac{\dot{\lambda}}{\lambda'} =$
(taking $\dot{\lambda}$ positive instead of negative, as it ought to be when $\dot{l}$ is positive) $\gamma - \frac{r}{s} \cdot \frac{\dot{\lambda}}{\lambda'}$; and, consequently, $\frac{\dot{\gamma}}{\gamma} = 1 - \frac{r}{s\gamma} \cdot \frac{\dot{\lambda}}{\lambda'}$, when radius is unity, but $= m^2 - \frac{rm^2}{s\gamma} \cdot \frac{\dot{\lambda}}{\lambda' m^2}$, when radius is m. Now $\frac{\dot{\gamma}}{\gamma}$ may be considered as the increment of the hyperbolic log. of γ, and therefore, with its proper modulus, may represent the area which is the object of our investigation. We may suppose the other side of the equation to be the square of the cosine of the arc whose sine is $\left|\frac{rm^2}{s\gamma}\right|^{\frac{1}{2}} \times^2$. into the increment of the hyperbolic log. of λ', divided by the square of the radius; and if, instead of taking this log. with the hyperbolic, we take it with BRIGGS's modulus, we must then consider $\frac{\dot{\gamma}}{\gamma}$ as the increment of the log. of γ, according to the same system. But $\frac{\dot{\lambda}}{\lambda'}$ being equal to $\frac{s\dot{L}}{m^2}$ (when $\dot{L}$ is only one minute), it will vary as s; and, therefore, if its value be determined according to BRIGGS's system, when s is equal to radius, and be denominated v, its value in any other case will be expressed by $\frac{vs}{m}$.

Hence, to obtain the log. of the area gc, the quantity with which we are immediately concerned, we must find the log. value of $\frac{rm^2}{s\gamma}$, and divide it by 2; we must then take out the log. cosine of the arc whose log. sine is equal to the quotient; and, having multiplied it by 2, we must add the product to the constant log. of v (3,1015), and the log. tangent of the sup-

posed latitude, rejecting thrice the log. radius. But, if $\left|\frac{r\,m^2}{s\,\gamma}\right|^{\frac{1}{2}}$ be greater than radius, which must necessarily be the case when the azimuth is greater than a right angle, we must then consider $\frac{r\,m^2}{s\,\gamma} - m^2$ as the square of the tangent of the arc whose secant is $\left|\frac{r\,m^2}{s\,\gamma}\right|^{\frac{1}{2}}$, observing in other respects the directions before given. The quantities r and s are both employed in the first computation, from the result of which we also obtain γ; and, consequently, this operation will not be attended with much trouble.

The above instructions, it is manifest, are given upon the supposition of r and s having the same sign; but, if the declin. and latit. should not be of a similar denomination, then will our expression become $\sqrt{m^2 + \frac{r\,m^2}{s\,\gamma} \cdot \frac{v\,s}{m^2}}$, and we must consider $m^2 + \frac{r\,m^2}{s\,\gamma}$ as the square of the secant whose corresponding tangent is $\left|\frac{r\,m^2}{s\,\gamma}\right|^{\frac{1}{2}}$. With this exception, the process will be the same as when the tangents r and s are both affirmative.

The preceding *formula* naturally suggests to us another method of finding the log. area gc; and, as some perhaps may think this more eligible than either of the former, I shall take the liberty of explaining it. When the latit. is given, the area GC, it is obvious, must invariably preserve the same magnitude at all distances from the meridian; and, consequently, the area gc, which is proportional to it, must likewise remain constant. If, therefore, we can ascertain this area when the hour-angle is supposed to vanish, we may employ it when the sun is at any distance from noon. Let us now conceive the declin. to be equal to nothing; then will our expression

for the area gc become $\frac{\dot{\lambda}}{\lambda^2}$; and, consequently, (since the tangents of the azimuth and hour-angle vanish in the ratio of their sines, or of the sines of the opposite sides in the triangle alluded to before,) we shall have the area $GC = \frac{\dot{\lambda} m}{\lambda^2 l}$, when the sun is upon the meridian. But this area is always the same when the latitude is given, whatever be the sun's declination, and therefore may always be represented by $\frac{\dot{\lambda} m}{\lambda^2 l} = \frac{v s}{m} \times \frac{m}{l} = \frac{v m}{\lambda}$; and the area gc will be generally expressed by $\frac{v m}{\lambda} \times \frac{\text{cos. of merid. altit.}}{\text{cos. of declin.}}$, when the hour-angle does not exceed the limits which have been recommended. Hence, if we add together the constant log. 3,1015, the log. radius, and the log. cosine of the merid. altitude, and subtract from their sum the log. cosines of the latit. and declin. we shall obtain the log. value of gc.

It will be necessary, perhaps, to meet an objection which some may be inclined to urge against the method of deducing the hour-angle in terms of the cosine, when this angle is very small. But it should be recollected, that with the angle itself we have no immediate concern, the accuracy of our conclusion depending entirely upon the accuracy with which the area corresponding to any particular increment of time can be determined. Now this area, whatever be the sun's distance from the meridian, will be nearly proportional to the increment of the latit. and, consequently, its magnitude is totally unconnected with that of the hour-angle. A given error in the quantity which expresses this area will equally affect our conclusion, whether the angle be 2, or whether it be 20 degrees. But let us inquire what effect will actually be produced, by admitting

an error of half an unit in each of the log. cosines whose difference is equal to the area gc; and, of course, in some instances, an error of an unit in the area itself, upon any particular supposition of latit. and declin. We have only to ascertain the ratio which this area bears to unity; for the same ratio will the correction of the latit. bear to the error in our result. If the latit. for instance, be 50°, and the declin. 10°, on the same side of the equator with the latit. then, radius being unity, $\dot{z}$ (the increment of the hour-angle) will equal $\frac{\dot{L}}{\lambda T}$ $= \frac{\dot{L}}{\text{cos. } 50^{\circ} \times \frac{S}{T}} =$ (g being the sine of the hour-angle, and α the cos. of the altit.) $\frac{\dot{L}}{\text{cos. } 50^{\circ}} \times \frac{\sqrt{a^2 - g^2 b^2}}{g b} =$ 11 minutes nearly, when z is 5°; and we have seen that, at any other distance from the meridian, the incremental area will be of the same magnitude. Hence, subtracting the log. cosine of 5° from that of 5° 11′ 0″, we get the difference equal to 1938; and, consequently, the error in our approximation will be to the error in the assumed latit. as 1 : 1938, when the log. cosines are carried to seven places of decimals.

But, when the zenith distance of the sun, at his greatest altit. is very small, and there is moreover a considerable uncertainty with respect to the latit. this error will probably become of more importance, and we may find it necessary to guard against it. Now it is manifest that, by diminishing the multiple which the area gb is of the area gc, exactly in the same proportion we shall diminish this error; and I shall afterwards explain in what manner it may be accomplished. The same expedient is also calculated to prevent another species of inac-

curacy, to which, under similar circumstances, our process is liable.

If we are desirous of knowing how much our conclusion is affected by substituting the tangent of the hour-angle for the tangent of the azimuth, it may be done with the greatest facility. We have seen, that the area GC (fig. 1.) is to GB as one minute to the error in the assumed latitude. But our method supposes that gc (fig. 2.) is to gb as one minute to the correction required; and we must therefore estimate the difference betwixt these ratios, in order to ascertain how far the approximation is inaccurate. Let $EG = m \times FG$; and then, since GC may be considered as $= \frac{GD + FC}{2} \times FG$, and $GB = \frac{GD + EB}{2} \times EG = \frac{GD + EB}{2} \times m \times FG$, the former ratio will equal $\frac{GD + EB}{GD + FC} \times m =$ (according to the notation employed above) $m \cdot \frac{2T - m\dot{T}}{2T - \dot{T}} = m - m \cdot \frac{m-1}{2} \times \frac{\dot{T}}{T}$ nearly = the minutes contained in the computed error of latitude. Their difference will equal $m \cdot \frac{m-1}{2} \cdot \frac{\dot{T}}{T} - \frac{\dot{t}}{t} = m \cdot \frac{m-1}{2} \cdot \dot{z} \cdot \frac{\overline{1+T^2}}{t} - \dot{z} \cdot \frac{1+t^2}{t}$ $= m \cdot \frac{m-1}{2} \cdot \frac{T^2 - t^2}{t} \cdot \dot{z} = m \cdot \frac{m-1}{2} \cdot \frac{T^2 - t^2}{\lambda T t} \cdot \dot{L}$ ($\frac{\dot{L}}{\lambda T}$ being substituted for its equal $\dot{z}$) = the minutes contained in the error of our approximation. It will be more convenient, however, to express this difference independently of the azimuth. Now, preserving the notation before adopted, we have $T = \frac{S}{\Sigma} = \frac{g\delta}{a \times \frac{la - d}{\lambda a}} = \frac{t\gamma\delta\lambda}{la - d} = \frac{t\gamma\delta}{l\gamma\delta - d\lambda}$; therefore, $\frac{T}{t} = \frac{\gamma\delta}{l\gamma\delta - d\lambda}$, and $\frac{T}{t} - \frac{t}{T} \left(= \frac{T^2 - t^2}{Tt}\right) = \frac{\gamma\delta}{l\gamma\delta - d\lambda} - \frac{l\gamma\delta - d\lambda}{\gamma\delta}$

$= \frac{\gamma^2 \dot{\gamma}^2 - l^2 \gamma^2 \dot{\gamma}^2 + 2 d l \gamma \dot{\gamma} \lambda - d^2 \lambda^2}{\gamma \dot{\gamma} \times l \gamma \dot{\gamma} - d\lambda} = \frac{\gamma^2 \dot{\gamma}^2 \lambda^2 + 2 d l \gamma \dot{\gamma} \lambda - d^2 \lambda^2}{\gamma \dot{\gamma} \times l \gamma \dot{\gamma} - d\lambda.} = (d^2 \lambda^2$ being rejected, as incomparably less than the sum of the other terms,) $\lambda \cdot \frac{\gamma \dot{\gamma} \lambda + 2 d l}{l \gamma \dot{\gamma} - d\lambda} = \lambda \cdot \frac{\gamma + 2rs}{s\gamma - r}$; and, consequently, we have $m \cdot \frac{m-1}{2} \cdot \frac{T^2 - t^2}{\lambda T t} \cdot \dot{L} = m \cdot \frac{m-1}{2} \cdot \frac{29}{100000} \cdot \frac{\gamma + 2rs}{s\gamma - r}$ minutes. Hence it appears that, in the latitude of Cambridge, when the sun's declination is 2° north, and his distance from the meridian 5°, this error will be equal to $m \cdot \frac{m-1}{2} \cdot \frac{1}{4312}$ minutes of a degree. Let the assumed differ from the true latitude ten minutes, *i. e.* let $m = 10$, then will the error amount to half a second; and, if we suppose $m = 30$, the error will not exceed four seconds.

If, instead of varying the assumed latitude one minute, we vary it n minutes, in calculating the area gc, the above expression will be transformed into $m \cdot \frac{m-1}{2} \cdot \frac{29 \cdot n}{100000} \cdot \frac{\gamma + 2rs}{s\gamma - r}$, the increment of the latitude being, in this case, $n\dot{L}$. The error will consequently vary as $m \cdot \frac{m-1}{2} \cdot n$, when the declination, latitude, and time of observation are given; and, if we suppose the real to differ from the assumed latitude p minutes, this last expression will become $\frac{p}{n} \cdot \frac{\frac{p}{n} - 1}{2} \cdot n = p \cdot \frac{p-n}{2n}$, which varies as $\frac{p-n}{n}$, when p remains constant; and, of course, the error may be diminished in any proportion by diminishing this quantity $\frac{p-n}{n}$. Now, the increment of the latitude is always $= \lambda T \dot{z}$; and, therefore, since $\dot{z}$ is determined by the process explained above, we have only to ascertain the value of T, in order to approximate nearly to p. But the sine of the

azimuth is $= \frac{\xi^2}{a}$, which is known, and consequently the tangent may likewise be obtained. Having thus discovered a near value of p, or of the error in our first assumed latitude, we are enabled so far to correct it in our second hypothesis, and in a much greater degree to reduce the error in our conclusion.

As it will sometimes, though not often, be necessary to have recourse to this expedient, two short tables might be added, in order to facilitate the operation. The first might contain the log. cosines to every degree from the 15th to the 90th of the quadrant; and be so contrived, as likewise to exhibit the log. sine of any arc, as far as the 75th degree, expressed in minutes and seconds of time; that, by subtracting the log. cosine of the altitude from the log. sine of the hour-angle, (the cosine of declination being considered as equal to radius,) we might obtain the log. sine of the azimuth. This should be made one of the arguments of the second table; the other being the cosine of the latitude to every five degrees of the first sixty of the quadrant; and the table should give us the value of $15 \times \lambda T$, which, multiplied into the minutes contained in the error of time, would determine with sufficient exactness the quantity p. We might, indeed, with the same facility, compute the fourth table according to the mean value of the cosine of declination, if it could be supposed that such a degree of precision would ever be required.

Perhaps it would be advisable for the mariner, in order to avoid all distinction of cases, to calculate, in every instance, his incremental area with the lat. varied ten minutes, instead of one; and then he would always be secure of a result sufficiently correct for his purpose. In the case supposed above, if the real

were to differ from the assumed latitude 30 minutes, the error in his conclusion would not exceed the fourth part of a second.

We must observe, that if n (the approximate value of p, thus deduced) be the minutes by which the first assumed lat. is varied in our second hypothesis, we ought to multiply the correction, after it is taken from the 2d table, by this quantity; but, as n is always supposed to be a whole number, the additional trouble arising from this process can never be an object of the smallest consideration. The 2d table, however, may be as conveniently applied to the case, where the lat. is varied ten minutes, as where it is varied only one. The logarithms which form the argument are the same in both cases, except that the index in the former is less by unity than in the latter.

Another species of inaccuracy originates in our conceiving the fluxion of the lat. to vary as $T\dot{z}$, instead of $\lambda\, T\dot{z}$; and it may be useful to ascertain how much our conclusion is affected by this circumstance. We suppose, in fact, that the increments of the lat. corresponding to the finite times gf and ge are to each other as the areas gc and gb drawn into the cosine of the first lat.; whereas, in strict propriety, they are to each other as the sum of all the elements of these areas drawn into the cosines of the latitudes belonging to each, *i. e.* (if $ge = m \times gf$) we consider these increments as being in the ratio of $gc \times \lambda : gb \times \lambda$, instead of $gc \times \frac{2\lambda + \dot{\lambda}}{2} : gb \times \frac{2\lambda + m\dot{\lambda}}{2}$. Hence

$$\frac{gb \,.\, \frac{2\lambda + m\dot{\lambda}}{2}}{gc \,.\, \frac{2\lambda + \dot{\lambda}}{2}} - \frac{gb}{gc} = m \,.\, \frac{m-1}{2}\, \frac{\dot{\lambda}}{\lambda^2} = m \,.\, \frac{m-1}{2} \,.\, s\dot{\mathrm{L}}\ (\mathrm{rad.} = 1)$$

$= m \cdot \frac{m-1}{2} \cdot \frac{2gs}{100000} =$ the minutes contained in the error proceeding from this cause. When the real latitude, for instance, is 52°, and the assumed 51° 50′, this error will scarcely amount to a single second. But, if either the cosine of the latitude should be so small, or the difference betwixt the supposed and true latitude so great, as to render this error of any importance, we may prevent it by the same means that were recommended in the preceding case. In computing the incremental area gc, we must correct our first hypothesis respecting the latitude with the assistance of the third and fourth tables; and, for the reasons assigned above, the cause of this inaccuracy will be removed.

There is still another part of our process, which will sometimes be the source of a small error. We are directed to distribute the whole increment of the time betwixt the two observations, by equalizing the areas corresponding to each in the "*figura tangentium;*" whereas they can only be considered as perfectly equal in the original curve whose ordinates are the tangents of the azimuth. It is supposed, in reality, that gc bears to GC the same ratio which gc bears to GC; or (which is nearly the same thing) that gd is to GD as gd to GD: we must therefore estimate the difference betwixt these ratios, and we shall easily deduce the amount of this error. Let us retain the notation employed above, and moreover let Γ represent the cosine, and τ the tangent of the hour-angle at the observation farthest from noon; $\dot{A}$ the difference betwixt the hour-angles, and $\dot{y}$ the excess of γ above Γ: then, from what has already been demonstrated, we have $\frac{gd}{GD} = \frac{l\gamma\dot{\lambda} - d\lambda}{\gamma\dot{\lambda}}$, and $\frac{gd}{GD} = \frac{l\Gamma\dot{\lambda} - d\lambda}{\Gamma\dot{\lambda}}$;

consequently, $\frac{g\,d}{GD} - \frac{g\,d}{GD} = \frac{d\lambda\dot{\gamma}}{\gamma\Gamma\dot{\lambda}}$ (d being neglected) will express the part of *BG*, or BG, (when they are supposed to be very small,) by which bg exceeds bg. Let this be multiplied into $\frac{GD}{g\,d}$, and it will equal $\frac{d\,l\dot{\gamma}}{\Gamma.l\gamma\dot{\lambda} - d\lambda} = \frac{r\dot{\gamma}}{\Gamma.s\gamma - r}$ = the part of bg itself by which it is greater than bg. But our method of equalizing the areas will take from bg a portion of this excess bearing to the whole the ratio of $t : t + \tau$; and, consequently, bg will be made too small by $\frac{r\dot{\gamma}}{\Gamma.s\gamma - r} \times \frac{t}{t+\tau} = \frac{rt\dot{\Lambda}}{2.s\gamma - r}$ nearly. Hence $\frac{mrt\dot{\Lambda}}{2.s\gamma - r}$ will express the minutes contained in the error arising from this cause.

The same conclusion may be deduced from the formula $\frac{\dot{\gamma}}{\gamma} = \sqrt{1 - \frac{r}{s\gamma}} \cdot \frac{\dot{\lambda}}{\lambda'}$ = the area gb, if the increment of the time be equal to ge. For the same reasons, $\frac{\dot{\Gamma}}{\Gamma} = \sqrt{1 - \frac{r}{s\Gamma}} \cdot \frac{\dot{\lambda}}{\lambda'}$ = the area gb, if the increment of the time be represented by ge. Hence $gb - gb = \frac{\dot{\gamma}}{\gamma} - \frac{\dot{\Gamma}}{\Gamma} = \frac{r\dot{\gamma}}{s\gamma\Gamma} \times \frac{\dot{\lambda}}{\lambda'}$, and $\frac{gb - gb}{gb} \times \frac{t}{t+\tau} = \frac{r\dot{\gamma}}{\Gamma.s\gamma - r} \times \frac{t}{t+\tau}$ = that part of the whole area gb by which it is made to exceed its proper magnitude; and $\frac{mrt\dot{\Lambda}}{2s\gamma - r}$ = the minutes contained in the error which we are investigating. If, for instance, the latitude be 52° 12′, and the declination 2°, both of the same kind, and one of the altitudes be taken at the distance of 5°, the other at the distance of 10°, from the meridian, this error will amount to $\frac{1}{10000}$th part of the whole difference betwixt the real and the assumed

latitude. Hence, if this difference be ten minutes, the error in the approximation will not be more than $\frac{1}{17}$th part of a second; and, if the difference be one degree, the error will only be six times greater than in the former case. It will therefore always be inconsiderable, except when the latitude and declination are nearly equal, and on the same side of the equator.

But, if this cause should be likely to produce an effect of some importance, we may prevent it by computing an incremental area with the least altitude likewise, and the latitude increased or diminished by a minute, and then taking a mean betwixt them both for the magnitude of the area gc. Thus we shall generally obtain a conclusion sufficiently exact; but, if we are desirous of rendering it perfectly so in every instance, we must divide the difference betwixt the real and apparent intervals, in such a manner that the areas assigned to the two observations may be to each other in the same ratio as their correspondent incremental areas. This division will be readily dispatched by making the difference betwixt the logarithms of the two first terms, in the proportion (taken from the 1st table) equal to the difference betwixt the logarithms of the two last, which are given when either of the formulæ $\overline{m^2 - \frac{r\,m^3}{s\,\gamma} \cdot \frac{v\,s}{m^3}}$, or $\frac{v\,m}{\lambda} \times \frac{\text{cos. of merid. alt. } (\mu)}{\text{cos. of declin. } (\delta)}$, is employed; but must be found, if the first method of computing the area gc be adopted. Suppose that a and b, representing the logs. of the areas gb and gb in the table, appear from inspection to exceed each other nearly as much as the logs. of the areas gc and gc; let c and d be the two next terms in succession, and let m and n be the logs. of the quantities expressing the incremental areas; then will

$\overline{a \sim b} \sim \overline{c \sim d} : \overline{a \sim b} \sim \overline{m \sim n} :: \overline{a \sim c}$: the quantity to be added to, or subtracted from, a, to make it of the required magnitude. We have then only to subtract m from this corrected value of a, to proceed with the remainder to the second table, and take out the corresponding error in the assumed latitude. This precaution, however, can very seldom be necessary; and, even when it is deemed advisable to adopt it, the division may be performed with so little regard to exactness as to render the process easy and expeditious.

But we are not to conclude that, because this inaccuracy, and also the two first species that were considered, are likely to be introduced, when the latitude and declination are nearly equal and of the same kind, they will therefore unavoidably exist in these circumstances. On the contrary, they may always be prevented, when the weather is favourable, by making the observations within a smaller distance from the meridian. We have only to wait till the increase of the sun's altitude becomes so slow as not to produce a visible separation of the limbs in two or three seconds, and then we may be assured that the azimuth is small, and consequently that none of these errors will be considerable.

I have hitherto supposed that this method is only to be adopted, when the sun, at each observation, is within fifteen degrees of the meridian; or (to speak more accurately) when both the azimuth and the hour-angle are so small that we may consider their tangents as bearing a given ratio to each other; and, indisputably, these limits should never be transgressed, when it can possibly be avoided; for we have seen (page 79th) that, whatever be the method employed, the smaller the

hour-angle, the greater is the exactness with which the lat. is determined. Sometimes, however, it will be impossible to make both, or perhaps either of our observations within the distance which I have recommended; but, even in these cases, our rule may be conveniently applied. It has already been demonstrated that we can never be subject to any material error in consequence of the inequality of the areas gb and gb, except when the zenith-distance of the sun, at his meridian altitude, is very small; and, for this case, an effectual remedy has been provided. We need not, therefore, make any farther remarks upon this species of inaccuracy.

But perhaps it will be imagined, that because we still continue to suppose the areas of the "*figura tangentium*" to represent the increments of the latitude, a considerable error will be introduced. We can easily prove, however, that in consequence of the increment of the time being so much diminished by increasing the distance from noon, this error will seldom be of moment enough to claim our attention. Let T be the tangent of the azimuth, and τ the tangent of the hour-angle at the observation farthest from noon; then it follows, from what has been demonstrated before, that the ratio of gb to gc will exceed the ratio of GB to GC, and, consequently, the ratio of gb to gc, (supposed to be of a proper magnitude,) by $m \cdot \frac{m-1}{2} \cdot \frac{T+2\tau s}{sT-\tau}$ L. Hence it is evident that, by diminishing one of these ratios and increasing the other, as we are directed in the present case, till they become equal, we augment the latter by a portion of this difference expressed by $\frac{t}{t+\tau}$, and, on this account, it will be made too great by $m \cdot \frac{m-1}{2} \cdot \frac{t}{t+\tau} \cdot \frac{T+2\tau s}{sT-\tau}$ L.

But the ratio of gb to gc will itself exceed the ratio of GB to GC, its proper value, by $m \cdot \frac{m-1}{2} \cdot \frac{\gamma + 2rs}{s\gamma - r} \cdot \dot{L}$; and, by the method of equalizing the ratios, it is suffered to retain a portion of this difference expressed by $\frac{\tau}{t+\tau}$; and, therefore, it will be made too great by $m \cdot \frac{m-1}{2} \cdot \frac{\tau}{t+\tau} \cdot \frac{\gamma + 2rs}{s\gamma - r} \cdot \dot{L}$. The sum of these quantities, $m \cdot \frac{m-1}{2} \cdot \frac{29}{100000} \cdot \frac{1}{t+\tau} \cdot \frac{t\Gamma + 2rst}{s\Gamma - r} + \frac{\tau\gamma + 2rs\tau}{s\gamma - r}$, will determine, in minutes of a degree, the amount of the whole error, when the incremental area gc represents one minute of latitude. But if, in our second hypothesis, the latitude be varied n minutes, instead of one, and p be the correction required, the above expression becomes $p \cdot \frac{p-n}{2n} \cdot \frac{29}{100000} \cdot \frac{1}{t+\tau} \cdot \frac{t\Gamma + 2rst}{s\Gamma - r} + \frac{\tau\gamma + 2rs\tau}{s\gamma - r}$ minutes. Hence, in the lat. of Cambridge, when the declination is 2° north, and the sun's distance from the meridian at one observation is 5°, and at the other 45°, p being equal to 10, and n equal to unity, the error will amount to little more than half a second.

But, whenever it is judged necessary to guard against this species of error, we need only diminish the value of $\frac{p-n}{n}$ in the last expression; and this will readily be effected by dividing the difference betwixt the real and computed intervals of time, in such a manner that the portions belonging to the two observations may be to each other inversely as the quantities taken out of the fourth table, according to the instructions before delivered. When the portion belonging to the greatest altitude is thus obtained, we can immediately deduce the correction to be made in the first assumed latitude, and then proceed to calculate

the incremental areas. Let us suppose, for instance, the difference betwixt the intervals to be q, and the quantities deduced from the 4th table to be a and b; then will $\frac{a^2bq}{a^2+b^2}$ = the minutes by which the former latitude is to be varied.

If, however, there be any considerable uncertainty respecting the latitude, I would recommend the following method of obtaining an approximate value of it, before we begin the process for investigating its real magnitude. This will effectually preclude, in almost every instance, the various errors that have been described. The increment of the altitude is equal to $\lambda S\dot{z}$, and, therefore, when the azimuth is so small that its sine varies nearly as the arc, the whole increase of the altitude, whilst the sun is moving to the meridian, will be $\lambda z \times \frac{S}{2} = \frac{z^2\lambda\dot{\delta}}{2a}$, the sine of the hour-angle being considered as equal to the arc itself. Now the first of the two tables which have just been explained, will enable us to find the value of $\frac{z^2}{a}$, by subtracting the log. cosine of the altitude from twice the log. sine of the hour-angle; and a third table might be added, to furnish us with the whole expression $\frac{z^2\dot{\delta}\lambda}{2a}$, (according to the mean value of $\dot{\delta}$,) one of its arguments being the quantity $\frac{z^2}{a}$ already determined, and the other the supposed latitude. It might, perhaps, be advisable to add another column to the first of these tables, containing twice the logarithmic sine of the hour-angle, as this would in some measure abridge the operation. We should find it more convenient too, if the last table were to give us the complement of the arc whose value is $\frac{z^2\dot{\delta}\lambda}{2a}$, rather than the arc itself; because it

would then only be necessary to subtract the observed altitude from this complement, and we should immediately deduce the zenith distance of the sun, when he had arrived at the meridian. This being ascertained, we should have no farther difficulty in finding the latitude to be adopted in the subsequent computation. The exactness of the conclusion which is derived from this process, will necessarily depend upon the degree of certainty with which the time is given when the observation is made. An error in time may arise, both from an irregularity in the going of the clock and a small inaccuracy in estimating the difference betwixt the longitude of the place where the altitude is now taken, and that where the time was last determined; but these causes, it is evident, can seldom be very considerable. We may generally, I think, be sure of the time within two or three minutes.

The principle upon which this approximation depends will open to us a more compendious way of finding the area $g\,b$, and which may always be pursued with advantage, when one of the altitudes can be taken at a small distance from the meridian. In the higher latitudes, indeed, the hour-angle may amount to five or six degrees; but, when the latitude and declination approach towards an equality, and are of the same denomination, this angle must be restrained within narrower limits. If, instead of deducing the meridian altitude from the altitude observed, according to the directions before given, we consider it as being actually equal to the meridian altitude, and employ the latitude resulting from this hypothesis in computing the hour-angle, this angle must necessarily be found equal to nothing. We may, therefore, spare ourselves the trouble of performing this part of the operation, and need only calculate the time belonging to the second observation, which is supposed to be made when the sun is at a greater

distance from the meridian. The error in the assumed latitude will, in this case, be equal to the whole increase of the altitude betwixt the time of observation and noon; and from this consideration we may be enabled, in any particular instance, to discover at what distance from the meridian this plan may be safely adopted. We can extend it also to any number of observations that happen to be made within the proper limits, by connecting them successively with one on the opposite side of the meridian, and thus determining the areas *g b* corresponding to each. Having afterwards calculated an incremental area with any of the assumed latitudes varied one minute, we must subtract its logarithm from the logarithmic value of each of the areas *g b*, and we shall discover, by means of the second table, the correction to be applied to each of the supposed latitudes.

But the method which has just been explained might always be safely adopted, within the limits prescribed to the former rule, if, in computing the incremental area, we were to employ the latitude obtained by means either of the 3d and 4th, or of the 3d and 5th tables. This precaution would entirely prevent the error which might arise from the inequality of the ratios of *GC* to *GB*, and *g c* to *g b*, when the difference betwixt the real and the assumed latitude was very considerable. The error which is occasioned by the disparity of the hour-angles, might be obviated by the same means that were practised in the first method. There is only one objection, indeed, to this rule's being exclusively adopted, when either of the altitudes is taken within the distance at first recommended. It would be necessary, in that case, to extend the table much farther than we are obliged to do, if we adhere to the former rule; but the labour is so much abridged by this plan, that I am doubtful

whether its advantages might not be more than sufficient to compensate this inconvenience. The table should at least be carried to such a length as would enable us to proceed in this manner, whenever an observation was made within five or six degrees of the meridian.

I must not forget to observe, before I conclude the theory, that although I have directed the altitudes to be taken on different sides of the meridian, it is by no means requisite that we should invariably adhere to this precept. We have seen the reason, indeed, why it is expedient, in most instances, to prefer this method, as being generally calculated to produce a much greater degree of exactness in the result. This, however, is not always the case; for, if one of the observations be made beyond the distance originally prescribed, it is of little importance whether the second altitude be taken on the same side of the meridian, or not. But it will sometimes be impossible to make the observations on different sides of noon; and hence it becomes necessary to inquire in what manner the real latitude may be discovered in these circumstances. The clock gives us the interval betwixt the observations equal to $\overline{ae} - \overline{ae}$; and by computation we find $\overline{ag}$ and $\overline{ag}$, and thence we deduce $\overline{eg} - \overline{eg}$, the difference betwixt the errors in time. Having then assumed, without any regard to accuracy, two portions of time, corresponding to the two observations, whose difference is the same as the difference betwixt the errors before determined, and which are to each other in the inverse ratio of the hour-angles, we must increase or diminish them both equally, till we get the areas in the first table of the same magnitude, and then we may conclude that we have obtained the proper value of each. The directions which have been given for the prevention of errors in the

former case, when the altitudes are taken on different sides of the meridian, are very easily accommodated to the present; and it would therefore be superfluous to bestow any farther consideration upon them.

From a review of the inaccuracies to which this method, in particular cases, may be liable, it appears that none of them can ever be of sufficient importance to affect the mariner. If he only computes the time with each of the altitudes and the latitude by account, and an incremental area with the greatest altitude and the former latitude varied ten minutes, the correction will generally be deduced within much less than a second, and, in the most unfavourable circumstances, within a minute, of the truth. But the astronomer, in every instance, even when the latitude and declination are nearly equal and of the same kind, by adopting the precautions which have been recommended, may be assured of a result perfectly exact. If, however, he should entertain any doubts upon this point, he might easily compute a second value of the incremental area with the latitude already determined; and this, it is evident, would necessarily produce a conclusion not less accurate than if it were obtained from the direct method.

The most satisfactory way of proving the utility of this rule, will be to suppose a particular latitude and declination; with these to compute the altitudes, when the sun is at two given distances from the meridian; and thence to deduce the latitude, by an application of our own principles.

EXAMPLE I.

Let the latitude be $54^{\circ}\ 27'\ 50''$ north, and the declin. when the first altit. is taken, $2^{\circ}\ 22'\ 32''$, and, when the second is taken,

2° 21′ 35″, (of the same denomination as the latit.) one observation being made in the morning, when the sun is 5° 3′ 22″, and the other in the afternoon, when he is 10° 1′ 10″, distant from the meridian. The altitude in the former case will be found equal to 37° 44′ 52″; and in the latter to 37° 15′ 20″.

	Observation 1st. Lat. 54° 15′ 0″		Lat. 54° 16′ 0″	Observation 2d. Lat. 54° 15′ 0″
Log. of a =	9,7868838	-	9,7868838	- 9,7820217
— d =	8,6175181	-	8,6175181	- 8,6146155
— l =	9,9093281	-	9,9094190	- 9,9093281
— $d\,l$ =	18,5268462	-	18,5269371	- 18,5239436
$-\frac{a\,m^3}{d\,l}$ =	21,2600376	-	21,2599467	- 21,2580781
$-\sqrt{\frac{a\,m^3}{d\,l}}$ =	10,6300188	-	10,6299733	- 10,6290390
Log. tang. =	10,6177463	-	10,6176983	- 10,6167094
2 × log. tang. =	21,2354926	-	21,2353966	- 21,2334188
Log. of r =	8,6178915	-	8,6178915	- 8,6149839
— s =	10,1427296	-	10,1429961	- 10,1427296
Computted log. of y =	9,9961137	-	9,9962842	- 9,9911323
True — =	9,9983068	-	- -	- 9,9933253
Area $g\,b$ =	21931	-	9,9961137	- Area g b = 21930
			Area $g\,c$ = 1705	

Hence, $\frac{g\,b}{g\,c} = \frac{21931}{1705} = 12'\ 51''$, and latit. = 54° 27′ 51″.

If we employ the formula $\frac{\dot{y}}{y} = \overline{m^3 - \frac{r\,m^3}{s\,y}} \cdot \frac{v\,s}{m^3}$ in computing the area $g\,c$, we shall obtain, very nearly, the same result.

Log. of r =	8,6178915
— s =	10,1427296
— γ =	9.9961139
— $s\gamma$ =	20,1388435
$-\frac{rm^3}{s\gamma}$ =	18,4790480
$-\sqrt{\frac{rm^3}{s\gamma}}$ =	9,2395240
Log. cosine =	9.9933560
2 x log. cos. =	19,9867120
Const. log. =	3,1015
Log. s =	10,1427296
— gc =	3,2309416
— gb =	4,3410188
Difference =	1,1100772, producing in tab. 2d. 12′ 52″
	Lat. corrected = 54° 27′ 52″

But there is no occasion to write down the logs. of r, s, and γ, again: we may add together the logs. of s and γ, and subtract their sum from the log. of rm^3 at the same time, as they stand in the former operation, and this will render the latter process extremely short. Let us resume the preceding example, and compute the area gb by the second method, and the area gc by the formula $m^3 - \sqrt{\frac{rm^3}{s\gamma}} \cdot \frac{vs}{m^2}$.

Observation 1st.	Observation 2d.	Complement of alt. 52° 15′ 8″
Lat. 54° 37′ 40″.	Lat. 54° 37′ 40″.	Declin. 2 22 32
		Lat. 54 37 40
	Log. of a = 9,7820217	
	− d = 8,6146155	
	− l = 9,9113753	
	$-\frac{a m^2}{d l}$ = 21,2560309	
	$-\left\lvert\frac{a m^2}{d l}\right\rvert^{\frac{1}{2}}$ = 10,6280154	
	Log. tang. = 10,6136259	
	2 × log. tang. = 21,2272518	Log. of $\frac{r m^2}{2 y}$ = 18,4711836
	Log. r = 8,6149839	$\left\lvert\frac{r m^2}{2 y}\right\rvert^{\frac{1}{2}}$ = 9,2355918
	− s = 10,1487823	Log. cosine = 9,9934771
Computed log. y = 10,0000000	9,9950180	2 × log. cos. = 19,9869542
True − = 9,9983068	9,9933253	Const. log. = 3,1015
Area $g b$ = 16932	Area $g b$ = 16927	Log. area $g c$ = 3,237
$g b + g b$ = 33859		
Mean value of $g b$ = 16929		
Whose log. (from tab. 1st) − = 4,229		
$g c$ = 3,237		
Difference = 1992	producing from tab. 2d −	9′ 49″
		Lat. corrected = 54° 27′ 51″

EXAMPLE II.

Let the real latitude be 10° 0′ 0″, and the two observations be made on different sides of noon; one when the sun's distance from the meridian is 5° 0′ 0″, and the other when his distance is 10° 0′ 0″; the declin. in the former case being 7° 40′ 40″, and in the latter 7° 39′ 48″, of the same kind as the

latitude. Then will the greatest altitude be 84° 32′ 28″, and the least 79° 50′ 48″.

	Observation 1st. Lat. 10° 10′ 0″	Observation 2d. Lat. 10° 10′ 0″
Log. of a =	9,9980259	9,9980259	9,9931449
—— d =	9,1258124	9,1258124	9,1249212
—— l =	9,2467746	9,2460695	9,2467746
—— $\frac{am^3}{dl}$ =	21,6254389	21,6261440	21,6214491
—— $\sqrt{\frac{am^3}{dl}}$ =	10,8127194	10,8130720	10,8107245
Log. tang. =	10,8075134	10,8078745	10,8054699
2 × log. tang. =	21,6150268	21,6157490	21,6109398
Log. of r =	9,1297253	9,1297253	9,1288160
—— s =	9,2536477	9,2529200	9,2536477
Computed log. of y =	9,9983978	9,9983923	9,9934035
True —— =	9,9983442	—	9,9933515
	Area gb = 536	9,9983978	Area gb = 520
		Area ge = 55	

$\frac{gb+gb}{2}$ = 528. Hence, correction = $\frac{528}{55}$ = 9′ 34″, and lat. corrected = 10° 0′ 26″.

The difference, however, betwixt the real and the corrected lat. would only have amounted to 19″, if we had determined the area gb by the 1st table, instead of taking a mean betwixt the two areas for its proper value. This error might have been expected from the near approach of the lat. to the declin. and ought therefore to have been guarded against. It proceeds from the three causes of inaccuracy which, I have shewn, must necessarily be combined in these circumstances. The remedies

to be applied are well known. We must find an approximate value of p (page 89) by means of the 3d and 4th tables, and compute an incremental area with the least, as well as with the greatest altitude. If this plan had been pursued, the latitude would have been ascertained within less than a second of the truth. Or, if the weather had been favourable, we might have prevented the error as effectually, by making the observations when the hour-angles were much less, and approached nearer to an equality.

N. B. The log. sines and tangents of the declin. and lat. to be employed in all the operations should be taken out at the same time. But, when the first method of computing the incremental area is adopted, we may avail ourselves, with considerable advantage, of the following expedient. Instead of taking the whole log. sine and tangent of the new lat. take the increments of each, or the difference betwixt their respective values in the two suppositions; and find the increment of the log. tangent corresponding to the increment of the log. secant $\frac{am^2}{dl}\Big|^{\frac{1}{2}}$. This may readily be effected, when the log. tangent is deduced from the log. secant in the former process, by taking the rate of increase belonging to each, and thence inferring by an easy proportion the proper increment of the log. tangent in the latter case. The difference betwixt this increment and the increment of r is the value of the area gc. An instance of the method thus improved shall be given in the next example.

EXAMPLE III.

Let us suppose the true lat. to be 54° 27′ 50″, and two observations to be made in the afternoon—one when the sun's distance from the meridian is 4° 30′ 0″, the other when his distance is 45° 10′ 15″—the declin. in the former case being 8° 7′ 35″, and in the latter 8° 5′ 3″, on the same side of the equator with the latitude. Then will the greatest altitude be found equal to 48° 31′ 19″, and the least to 31° 20′ 25″.

	Observation 1st. Lat 54° 10′ 0″		Observation 2d. Lat. 54° 10′ 0″		Observation 1st. Lat. 54° 11′ 0″		Observation 2d. Lat. 54° 11′ 0″
Log. of	$a = 9.8379894$	-	9.7161022				
——	$d = 9.1503179$	-	9.1480707				
——	$l = 9.9088727$	-	9.9088727				
——	$\frac{a\,m^3}{d\,l} = 20,7787988$	-	$20,6591588$	Incr. of log. of $l =$	912	-	912
——	$\sqrt{\frac{a\,m^3}{d\,l}} = 10,3893994$	-	$10,3295794$	—— log. secant $=$	456	-	456
Log. tang.	$= 10,3498734$	-	$10,2758492$	—— tang. $=$ 2 × incr. of log.	545	-	584
2 × log. tang.	$= 20,6997468$	-	$20,5516984$	tang.	- $= 1090$	-	1168
Log. of	$r = 9.1547009$	-	9.1524081				
——	$s = 10,1413981$	-	$10,1413981$	Incr. of log. of $s =$	2662	-	2662
Computed log. of	$\gamma = 9.9958458$	-	9.8455046	Area	$gc = 1572$ ..	$gc =$	1494
True ——	$= 9.9986590$	-	9.8481862				
	Area $gb = 28132$		Area $gb = 26816$				

Now, by diminishing gb and gb, till the ratio of gc to gc becomes equal to the ratio of gb to gb, we should get $gb = 28126$, and $gc = 26735$. Hence $\frac{gb}{gc} = \frac{28126}{1572} = 17'\,53''$ the correction, and the latitude $= 54°\,27'\,53''$.

But the same conclusion would have been obtained indepen-

dently of the second incremental area, which I have demonstrated above can never be necessary, except when the zenith distance at noon is very small. The error of three seconds arises from a different cause, and might have been entirely excluded with the assistance of the 3d and 4th tables. We should, in fact, deduce from these tables an approximation of 22′; and, if we calculate the area gc with the lat. 54° 32′ 0″, it will be found equal to 34762, and consequently $\frac{28172}{36762} = 48''{,}62$, which, multiplied by 22, produces 17′ 49″,64 for the correction, and the lat. is found = 54° 27′ 49″,64.

It would have been still better, however, to have obtained a near value of the lat. in the first instance, by means of the 3d and 5th tables. The work might have been dispatched in rather less time, and the conclusion would have been as rigidly accurate.

It is obvious that, if the time when the first observation was made could have been ascertained within two or three seconds, the area gb might have been immediately found by either of the two methods which have been explained, without the assistance of a second altitude, or of the first table; and the latitude determined with much greater facility, and with sufficient exactness. When the azimuth indeed, as in the present case, does not exceed four or five degrees, an error of a second in time will not produce an error of more than a second in the result; and, as the azimuth decreases, the error in latitude, arising from a given error in time, will be diminished in the same proportion. In general, if $\dot{z}$ be the error in the assumed time, the error in the corrected latitude will be nearly equal to $15\,T\lambda\dot{z}$.

Let us then suppose the time to be given at the first observation, and let us determine the area gb by the second method, and the area gc by the formula $\frac{\upsilon m}{\lambda} \cdot \frac{\text{cos. of merid. alt. } (\mu)}{\text{cos. of declin. } (\delta)}$

Comp. of alt. =	46° 28′ 41″		
Declin. =	8 7 35		
Lat. assumed =	54 36 16		
		Log. of υ =	3,1015
		—— μ =	9,8604043
		—— $m\upsilon\mu$ =	22,9619043
Computed log. of γ =	10,0000000		
True —— =	9,9986591	—— δ =	9,9956171
		—— λ =	9,7628420
Area gb =	13409		
Log. of area gb =	4,127	—— Area gc =	3,2034452
—— gc =	3,203		
Difference =	,924 producing from tab. 2d.		8′ 23″½.
		Lat. corrected =	54° 27′ 52″½.

EXAMPLE IV.

The latitude, which is supposed in the last example to be 54° 27′ 50″, is that of Ravensworth, a village about five miles to the north of Richmond, in Yorkshire, where I resided some months in the summer of 1797. During that time, I neglected no opportunity of taking the sun's meridian altitude; and, accordingly, the latitude which is there assumed is the mean result of a considerable number of observations. Moreover, on the 8th of September, I took, with particular care, four altitudes,

when the sun was near the meridian; and with these I will now calculate the latitude, in order to apply the 1st and 2d tables, and likewise to exemplify the method of combining several observations together. The two first altitudes were taken in the morning, one at 19′ 24″, the other at 17′ 40″ before twelve o'clock, and were respectively 40° 49′ 6″, and 40° 50′ 51″; the two last in the afternoon, one at 17′ 53″, the other at 19′ 44″ past twelve o'clock, and were respectively 40° 49′ 50″, and 40° 48′ 18″. Now the longitude, found by the eclipses of Jupiter's satellites, is nearly 1° 40′ west of Greenwich; and hence the declin. appears to have been, at the two first observations, 5° 26′ 25″, and at the two last, 5° 25′ 47″; the interval betwixt the first and second, and betwixt the third and fourth, being so small that we may consider the declin. as remaining constant during each of these times.

	Observation 1st. Lat. 54° 29′	Observ. 2d. Lat. 54° 29′	Observ. 3d. Lat. 54° 29′	Observ. 4th. Lat. 54° 29′	
Log. of a =	9,8153538	9,8156096	9,8154610	9,8152367	
— d =	8,9768457	8,9768457	8,9760047	8,9760047	
— l =	9.9105959	9,9105959	9,9105959	9.9105959	
$-\frac{a\,m^3}{d\,l}$ =	20,9279122	20,9281680	20,9288604	20.9286361	Log. $\frac{r\,m^3}{s\,y}$ = 18,8300552
$-\left(\frac{a\,m^3}{d\,l}\right)^{\frac{1}{3}}$ =	10,4639561	10,4640840	10,4644302	10,4643180	$-\left(\frac{r\,m^3}{s\,y}\right)^{\frac{1}{3}}$ = 9.4150276
Log. tang. =	10,4366767	10,4368218	10,4372141	10,4370861	Log. cosine = 9.9847972
2 × log. tang. =	20,8733534	20,8736436	20,8744282	20,8741722	2 × l cos. = 19.9695944
Log. of r =	8,9788063	8,9788063	8,9779578	8,9779578	Log. v = 3,1015
— s =	10,1464648	10,1464648	10,1464648	10,1464648	— s = 10,1464648
Comput. log. y =	9.9986245	9,9989147	9,9988508	9,9985948.	Log. gc = 3,218

1st observation.	Time from the argument of 1st table -		18′ 14″, by clock	19′ 24″
4th, ———	———		18 26 ———	19 44
	Near log. value of gb from 1st table	3,296	36 40	39 8
	——— gb ———	3,276		36 40
		2)6,572	Error in time =	2 28
	First log. value of gb - -	3,286		
2d observation.	Time from the argument of 1st table -		16′ 12″, by clock	17′ 40″
3d ———	———		16 40 ———	17 53
			32 52	35 33
	Near log. value of gb from 1st table	3,294		32 52
	——— gb ———	3,264		
			Error in time =	2 41
		2)6,558		
	Second log. value of gb - -	3,279		
	Mean ——— - - -	3,282		
	Log. value of gc - - -	3,218		

Difference ,064 producing in 2d tab. 1′ 10″
Lat. corrected = 54° 27′ 50″

We now take the same example again, and apply the second method of computing the area gb, and the formula $\frac{m \nu \mu}{\lambda}$ in determining the area gc.

Comp. of alt. at 2d observ^{n}	49°	9′	9″	Comp. of alt. at 3d observ^{n}	49° 16′ 10″
Declin.	5	26	25	Declin.	5 25 47
Lat. =	54	35	34	Lat. =	54 35 57

	Observations 1st and 3d. Lat. 54° 35′ 57″	Observations 4th and 2d. Lat. 54° 35′ 34″	
Log. of a =	9,8153538	9,8232367	Log. ν = 3,1015
—— d =	8,9768457	8,9760047	—— μ = 9,8787
—— l =	9,9112212	9,9111868	$m\nu\mu$ = 22,9803
$-\frac{am^2}{dl}$ =	20,9272869	20,9280452	—— δ = 9,9980
$-\sqrt{\frac{am^2}{dl}}$ =	10,4636434	10,4640226	—— λ = 9,7629
Log. tang. =	10,4363220	10,4367520	— Area gc = 3,2194
2 × log. tang. =	20,8726440	20,8735040	
Log. of r =	8,9788063	8,9779578	
—— s =	10,1483229	10,1482204	
Computed log. of y =	9,9997732	9,9996822	
True —— =	9,9984422	9,9983881	
Area gb =	13310	12941	
Computed log. of Γ =	10,0000000	10,0000000	
True —— =	9,9986765	9,9987084	
Area gb =	13235	12916	
$\frac{gb+gb}{2}$ =	13272	12928	
Log of $\frac{gb+gb}{2}$ =	4,1229	4,1115	
—— gc =	3,2194	3,2194	
Difference =	,9039	,8921	
Hence, correction from tab. 2d =	8′ 1″	7′ 48″	
Lat. corrected =	54° 27′ 56″	54° 27′ 44″	

Mean corrected lat. = 54° 27′ 50″

REMARKS.

1. All the altitudes that are taken on the same side of noon, tend only to correct the error which may be supposed to exist in the greatest of these altitudes, and can have no effect in removing any inaccuracy to which the greatest altitude on the other side of the meridian may be subject. Hence we must take more than one altitude on each side of noon, if we are desirous of obtaining a very exact conclusion.

2. When some of the observations are made in the morning, and others in the afternoon, the smaller the hour-angle, in every instance, the more favourable it will be for our purpose. But, if we cannot procure an altitude on each side of the meridian, we ought to make one observation when the hour-angle is as large as possible, and with this all the rest should be separately combined. We must be cautious, however, not to let the sun be too near the horizon, lest the apparent altitude should be affected by the uncertainty of the refraction.

3. If the clock were to furnish us with the true time, we might combine together any two observations made within the proper limits, without applying to the first table, and deduce a very exact correction. Should there even be a small error in the supposed time, we might still proceed in the same manner, without being liable to any material inaccuracy, provided the difference betwixt the hour-angles was not very considerable. The error, indeed, occasioned by adopting this method of finding the two areas, and taking a mean betwixt them for the value of $g\,b$, may easily be determined in any particular case. If t and t' be the respective tangents of the smaller and greater hour-angles, and i their difference; $\dot{z}$ the error of the clock in

minutes of time; and $\dot{Z}$ the whole error in the time computed with the greater altitude; then will the error in the result be to the whole correction of the latitude $(\dot{L})$ :: $\frac{t\dot{z}-t\dot{z}}{2} : t\dot{z}$ :: $\frac{t\dot{z}}{2} : \frac{t\dot{L}}{15T\lambda}$ :: $\frac{t\dot{z}}{2} : \frac{t\gamma\flat - d\lambda}{15\lambda\gamma\flat} \times \dot{L}$ :: $7{,}5 \times t\dot{z} : s - \frac{r}{\gamma}\dot{L}$. This error will consequently be equal to $\frac{7{,}5\,t\dot{z}}{s-\frac{r}{\gamma}}$; and hence it appears that, if we had pursued this method in the last example, and there had been an error of a minute in the time given by the clock, there would not have been an error of a single second in the conclusion.

4. If the time were determined by equal altitudes, and one of them were to be employed in computing the area $g\,b$, it is manifest that we should entirely exclude the error which has just been considered. It would be necessary, however, in order to correct by the second observation any inaccuracy that may have occurred in reading off at the first, to move the index, and then bring it apparently to the same position again, before we proceeded to take the second altitude.

EXAMPLE V.

On the 30th of January, 1799, the following altitudes of the sun's lower limb were taken in Trinity College, Cambridge; the height of the barometer being 29,6 inches, and of the thermometer 31 degrees.

Observed double alt. by the sextant.	Time by the clock.	Time from noon by the sun.	Log. cos. of the hour-angle.	Lat. from each pair of corresponding altitudes.
39° 35′ 19″	11h 21′ 0″	23′ 40″	9.9976800	52° 12′ 39″
40 7	20 51	20 49	8206	46
44 35	26 49	17 51	8681	45
48 5	29 13	15 27	9012	44
50 26	31 1	13 39	9229	40
52 30	33 0	11 40	9437	41
53 40	34 35	10 5	9579	41
55 19	36 46	7 54	9742	43
56 50	38 56	5 44	9864	38
57 20	41 4	3 36	9946	41
57 55	43 54	0 46	9999	41
57 50	46 40 —	2 0	9983	41
57 28	48 12	3 32	9948	41
57 0	49 59	5 49	9883	38
56 0	51 31	6 51	9806	43
55 15	53 0	8 20	9713	41
53 26	55 2	10 22	9556	41
51 5	57 44	13 4	9295	40
48 38	59 46	15 6	9057	44
45 17	12 2 9	17 29	8735	45
41 50	4 24	19 44	8388	46
35 10	8 20	23 40	7686	39
22) 1102 556			22) .2034190	
			9.9992463	
Error of adjust. 39 50 31				
- - 48				
2) 39 51 19			Area gb = 7537	
			Log. gb = 3,8772	
19 55 39				
Refra. — parall. 2 35				
19 53 4			Const. log. + log. rad. 13,1015	
Semi-diameter 16 18			Log. cos. merid. alt. 9,9726	
			23,0741	
Assumed merid. alt. 20 9 22				
Mean declin. 17 34 13			—— Declin. 9,9793	
			—— Lat. 9,7867	
37 43 35				
			Log. gc = 3,3081	
Assumed lat. 52 16 25				
Correction from tab. 2d. 3 43			Log. gb — log. gc = ,5691	
True lat. - 52 12 42				

not less accurate than the mean result from 22 meridian altitudes.

This process will require very little explanation. The first column contains the observed double altitudes, as they were read off from the sextant; the second contains the corresponding times given by the clock; the third contains the times from noon determined, with sufficient exactness, by taking half the interval betwixt the first and last observations, (in which the altitudes are equal,) and supposing this to be the time elapsed betwixt the first observation and the sun's reaching the meridian. The fourth column is formed of the log. cosines of the hour-angles taken immediately from the argument of the first table; and the last column exhibits the latitudes deduced from every two corresponding altitudes, and is intended to shew the agreement betwixt these results.

It is not necessary that we should employ the tables in this operation: for we may take the log. cosines of the hour-angles from TAYLOR's Logarithms, after the time is reduced into degrees, minutes, and seconds; and, by dividing the area $gb = 7537$, by the area $gc = 203$, (the natural number belonging to the log. 3,3081,) we shall obtain the correction required.

		0′ 12″	0′ 24″	0′ 36″	0′ 48″	1′ 0″	1′ 12″	1′ 24″	1′ 36″	1′ 48″	2
	9·99	2 · 2	2 · 2	2 · 2	3 · 3	3 · 3	3 · 3	3 · 3	3 · 3	3 · 3	3
16′	0′ 8941	43·42	73·72	908.892	036.014	136.108	217.185	287.249	348.304	401.352	45
—	8 923	43·42	73·72	912.896	039.018	139.112	221.189	290.253	351.308	405.356	45
—	16 905	43·43	74·73	916.899	043.022	143.116	225.192	294.256	354.312	408.360	45
—	24 887	44·43	74·73	919.903	046.025	146.120	228.196	297.260	358.315	411.364	46
—	32 869	44·44	74·73	922.907	050.029	150.123	231.200	301.264	361.319	415.368	46
—	40 850	44·44	75·74	926.910	053.033	153.127	235.203	304.267	365.323	418.371	46
—	48 832	45·44	75·74	929.914	057.036	156.130	238.207	307.271	368.326	421.375	47
—	56 813	45·45	75·74	933.918	060.039	160.134	241.210	311.275	371.330	425.378	47
17	4 795	45·45	76·75	936.921	063.043	163.137	245.214	314.278	374.334	428.382	47
—	12 776	46·45	76·75	940.924	067.046	166.141	248.217	317.282	378.337	431.386	47
—	20 757	46·46	76·75	943.928	070.050	169.145	251.221	321.285	381.341	434.389	48
—	28 737	46·46	77·76	946.931	073.053	173.148	254.225	324.289	384.344	438.393	48
—	36 718	47·46	77·76	949.934	077.057	176.151	257.228	327.292	387.348	441.396	48
—	44 699	47·47	77·76	952.938	080.060	179.155	261.231	330.296	390.351	444.400	49
—	52 679	47·47	78·77	956.941	083.063	182.158	264.235	333.299	394.354	447.403	49
18	0 659	48·47	78·77	959.944	086.067	186.161	267.238	336.303	397.358	450.407	49
—	8 639	48·48	78·77	962.947	090.070	189.165	270.241	339.306	400.361	453.410	50
—	16 619	48·48	79·78	965.951	093.073	192.168	273.245	343.309	403.365	456.413	50
—	24 599	49·48	79·78	968.954	096.077	195.171	276.248	346.312	406.368	459.417	50
—	32 578	49·49	79·78	971.958	099.080	198.174	279.251	349.316	409.371	462.420	51
—	40 558	49·49	80·79	975.960	102.083	201.178	282.254	352.319	412.374	465.423	51
—	48 537	50·49	80·79	978.964	105.086	204.181	286.257	355.322	415.378	468.426	51
—	56 516	50·49	80·79	980.967	108.090	207.184	288.261	358.325	418.381	471.430	51
19	4 495	50·50	80·80	984.970	111.093	210.187	291.264	360.329	421.384	474.433	52
—	12 474	51·50	81·80	987.973	114.096	213.190	294.267	363.332	424.387	477.436	52
—	20 453	51·50	81·80	989.976	117.099	216.193	297.270	366.335	427.390	480.439	52
—	28 431	51·51	81·80	993.979	120.102	219.196	300.273	369.338	430.394	483.442	53
—	36 410	51·51	82·81	996.982	123.105	222.199	303.276	372.341	432.397	486.445	53
—	44 388	52·51	82·81	998.985	125.109	225.202	306.279	375.344	435.400	488.449	53
—	52 366	52·51	82·81	001.988	129.111	228.205	309.282	378.347	438.403	491.452	53
20	0 344	52·52	83·82	004.991	131.114	230.208	312.286	381.350	441.406	494.455	54
—	8 322	53·52	83·82	007.994	134.117	233.212	314.288	383.353	444.409	497.458	54
—	16 300	53·53	83·82	010.997	137.120	236.215	317.291	386.356	446.412	500.461	54
—	24 277	53·53	83·83	012.000	140.123	239.217	320.294	389.359	449.415	502.464	55
—	32 255	53·53	84·83	015.003	142.125	242.220	323.297	392.362	452.418	505.467	55
—	40 232	54·53	84·83	018.006	145.129	244.223	326.300	394.365	455.421	508.470	55
—	48 209	54·54	84·83	021.009	148.131	247.226	328.303	397.368	457.424	510.473	55
—	56 186	54·54	85·84	024.011	151.134	250.229	331.306	400.371	460.427	513.476	56
21	2 163	55·54	85·84	027.015	154.137	252.232	334.309	403.374	463.430	516.479	56
—	12 139	55·54	85·84	029.017	156.140	255.235	336.312	405.376	465.432	519.481	56
—	20 116	55·55	85·85	032.020	159.142	258.237	339.314	408.379	468.435	521.484	56
—	28 092	55·55	86·85	035.022	162.145	261.240	342.317	411.382	471.438	524.487	57
—	36 068	56·55	86·85	037.025	164.148	263.243	344·320	413·385	473·441	526.490	57
—	44 044	56·56	86·85	040.028	167.151	266 246	347·323	416·388	476·444	529·493	57
—	52 020	56·56	86·86	043.031	169.154	268.248	349·326	418·391	478·446	531·496	57
22	0 7996	56·56	87·86	045.033	172.156	271.251	352·328	421·393	481 449	534·498	58
—	8 972	57·56	87·86	048.036	175·159	274·254	355·331	424·396	483 452	537·501	58
—	16 947	57·57	87·86	050.039	177.162	276.256	357·334	426·399	486 455	539·504	58
—	24 922	57·57	88·87	053.041	180.164	279.259	360.336	429.401	489 457	542.507	58
—	32 910	58·57	88·87	056 044	182.167	281.262	362.339	431.404	491.460	544·509	59
—	40 885	58·57	88 87	058.046	185.169	284.264	365.342	434.407	494 463	547·512	59
—	48 860	58.58	88.88	061.049	188.172	286.267	367·344	436.409	496.465	549·515	59
—	56 835	58.58	88 88	063.052	190.175	289.270	370.347	439 412	498.468	551·517	59
23	4 809	58.58	89 88	066.054	193 177	291.272	372.349	441.414	501.471	554·520	60
—	12 784	59 58	89.88	068 057	195.180	294.275	375.352	444 417	503.473	556.522	60
—	20 758	59·59	89 88	071.059	198.182	296.277	377.355	446.420	506.476	559.525	60
—	28 732	59 59	90.89	073 062	200.185	299 280	380.357	448 422	508.478	561.528	60
—	36 706	59·59	90.89	076.064	202.188	301.281	382.360	451.425	511.481	563.530	61
—	44 680	60 59	90.89	078.067	205.190	304.285	385·362	453.427	513.483	566.533	61
—	52 654	60 60	90 90	080.070	207.193	306.288	387.365	455.430	515.486	568.535	61

TABLE I.

To face page 116.

8″	2′ 0″	2′ 12″	2′ 24″	2′ 36″	2′ 48″	3′ 0″	3′ 12″	3′ 24″	3′ 36″	3′ 48″	4′ 0″	4′ 12″
3	3 · 3	3 · 3	3 · 3	3 · 3	3 · 3	3 · 3	3 · 3	3 · 3	3 · 3	3 · 3	3 · 3	3 · 3
52	450.395	494.434	534.468	571.500	606.530	638.556	669.581	698.605	725.627	751.647	776.666	799.084
56	453.399	497.438	537.472	575.504	609.534	642.561	672.586	701.609	728.631	754.651	779.670	802.688
60	457.403	501.441	541.476	578.508	613.537	645.564	676.590	704.613	732.635	757.655	782.674	806.692
64	460.407	504.445	544.480	581.512	616.541	648.568	679.594	707.617	735.639	761.659	785.678	809.697
68	463.410	507.449	547.484	584.516	619.545	651.572	682.597	711.621	738.643	764.663	788.683	812.701
71	466.414	510.453	551.488	588.520	622.549	655.576	685.601	714.625	741.647	767.667	792.686	815.705
75	470.418	514.457	554.492	591.523	626.553	658.580	688.605	717.628	744.651	770.671	795.690	818.709
78	473.421	517.460	557.495	594.527	629.557	661.584	692.608	720.632	747.654	773.675	798.694	821.712
82	476.425	520.464	560.499	597.531	632.560	664.587	695.613	723.636	750.658	776.679	801.698	824.716
86	479.429	523.467	563.502	601.535	635.564	667.591	698.616	726.640	754.662	779.683	804.702	827.720
89	483.432	526.471	567.506	604.538	638.568	670.595	701.620	729.644	756.666	782.686	807.706	830.724
93	486.436	530.475	570.510	607.542	641.571	673.598	704.624	732.647	760.669	785.690	810.710	833.728
96	489.439	533.478	573.513	610.545	644.575	677.602	707.627	735.651	763.673	788.694	813.713	836.732
00	492.443	536.482	576.517	613.549	647.578	680.606	710.631	738.655	766.677	791.697	816.717	839.735
01	495.446	539.485	579.520	616.552	651.582	683.609	713.635	742.658	769.680	794.701	819.721	842.739
07	498.450	542.489	582.524	619.556	653.585	686.613	716.638	744.662	771.684	797.705	822.724	845.743
10	501.453	545.492	585.527	622.559	656.589	689.616	719.642	747.665	774.688	800.708	825.728	848.746
13	504.457	548.495	588.531	625.563	659.592	692.620	722.645	750.669	777.691	803.712	827.732	851.750
17	507.460	551.499	591.534	628.566	662.596	694.623	725.649	753.672	780.695	806.716	830.735	854.754
20	510.463	554.502	594.537	631.570	665.599	697.627	728.652	756.676	783.698	809.719	833.739	856.757
23	513.466	557.505	597.541	634.573	668.603	700.630	731.656	759.679	786.701	812.722	836.742	859.761
26	516.470	560.509	600.544	637.576	671.606	703.633	733.659	762.683	789.705	814.726	839.746	862.764
30	519.473	563.512	603.547	640.580	674.609	706.637	736.662	765.686	792.708	817.729	842.749	865.768
33	522.476	566.515	606.551	642.583	677.613	709.640	739.666	767.689	794.712	820.733	844.752	868.771
36	525.479	568.518	608.554	645.586	680.616	712.643	742.669	770.693	797.715	823.736	847.756	870.774
39	528.483	571.522	611.557	648.589	682.619	714.647	745.672	773.696	800.718	825.739	850.759	873.778
42	531.486	574.525	614.560	651.593	685.622	717.650	747.676	776.699	803.722	828.743	853.761	876.781
45	534.489	577.528	617.561	654.596	688.626	720.653	750.679	779.703	805.725	831.746	855.766	876.785
49	536.492	580.531	620.567	656.599	691.629	723.656	753.682	781.706	808.728	834.749	858.769	881.788
52	539.495	583.534	623.570	659.602	694.632	726.659	756.685	784.709	811.732	836.753	861.772	884.791
55	542.498	585.537	625.573	662.605	696.635	728.663	758.688	787.712	814.735	839.756	863.776	886.794
58	545.501	588.540	628.576	665.608	699.638	731.666	761.692	789.716	816.738	842.759	866.779	889.798
61	547.504	591.543	631.579	667.611	702.641	734.669	764.695	792.719	819.741	844.762	869.782	892.801
64	550.507	594.547	633.582	670.614	704.644	736.672	766.698	795.722	821.744	847.765	871.785	894.804
67	553.510	596.549	636.585	673.618	707.647	739.675	769. 01	797.725	824.747	849.768	874.788	897.807
70	555.513	599.552	639.588	675.621	710.651	742.678	772. 04	800.728	827.750	852.772	876.792	899.810
73	558.516	602.555	641.591	678.624	712.653	744.681	774. 07	803.731	829.754	855.775	879.795	902.814
76	561.519	604.558	644.594	681.626	715.656	747.684	777. 10	805.734	832.756	857.778	881.798	904.817
79	563.522	607.561	647.597	683.629	718.659	749.687	779. 13	808.737	834.760	860.781	884.801	907.820
81	566.525	610.564	649.600	686.632	720.662	752.690	782. 16	810.740	837.763	862.784	886.804	910.823
84	569.528	612.567	652.603	689.635	723.665	755.693	785. 19	813.743	839.766	865.787	889.807	912.826
87	571.531	615.570	654.606	691.638	725.668	757.696	787. 22	815.746	842.769	867.790	891.810	915.829
90	574.534	617.573	657.608	694.641	728.671	760.699	790. 25	818.749	844.771	870.793	894.813	917.832
93	576.536	620.576	660.611	696. 44	730.674	762.702	792. 28	820.752	847.774	872.796	896.816	919.835
96	579.539	622.578	662.614	699. 47	733.677	765.705	795. 31	823.755	849.777	875.799	899.819	922.838
98	582.542	625.581	665.617	701.650	735.680	767.707	797. 33	825.758	852.780	877.802	901.822	924.841
01	584.545	627.584	667.620	704.652	738.682	770.710	799. 36	828.760	854.783	880.804	904.825	927.844
04	587.547	630.587	670.623	706.655	740.685	772.713	802.739	830.763	857.786	882.807	906.827	929.846
07	589.550	632.590	672.625	709.658	743.688	775.716	804.742	833.766	859.789	884.810	909.830	932.849
09	592.553	635.592	675.628	711.661	745.691	777.719	807.745	835.769	862.792	887.813	911.833	934.852
12	594.555	637.595	677.631	714.663	748.694	779.721	809.747	837.772	864.794	889.816	913.836	936.855
15	597.558	640.598	679.633	716.666	750.696	782.724	812.750	840.774	866.797	892.819	916.839	939.858
17	599.561	642.600	682.636	718.669	752.699	784.727	814.753	842.777	869.800	894.821	918.842	941.861
20	602.563	645.603	684.639	721.672	755.702	787.730	816.756	844.780	871.803	896.824	920.844	943.863
22	604.566	647.606	687.641	723.674	757.704	789.732	819.758	847.783	873.805	899.827	923.847	946.866
25	606.569	650.608	689.644	726.677	760.707	791.735	821.761	849.785	876.808	901.830	925 850	948.869
28	609.571	652.611	691.647	728.679	762.710	794.738	823.764	852.788	878.811	903.832	927.853	950.872
30	611.574	654.614	694.649	730.682	764.712	796 740	826.766	854.791	880.814	906 835	930.855	952.874
33	613.576	657.616	696.652	733.685	767.715	798.743	828.769	856.793	883.816	908.838	932.858	955.877
35	616.579	659.619	698.654	735.687	769.718	801.746	830.772	858.796	885.819	910.840	934.861	957 880

TAB. II.

— 1,176	0′ 4″
— ,875	0 8
— ,699	0 12
— ,577	0 16
— ,477	0 20
— ,399	0 24
— ,331	0 28
— ,273	0 32
— ,222	0 36
— ,176	0 40
— ,134	0 44
— ,097	0 48
— ,062	0 52
— ,030	0 56
,000	1 0
,028	1 4
,054	1 8
,079	1 12
,103	1 16
,125	1 20
,146	1 24
,166	1 28
,186	1 32
,204	1 36
,222	1 40
,239	1 44
,255	1 48
,271	1 52
,286	1 56
,301	2 0

TAB. III.

h.	′.	°	′	Log. of g or a.	Log. of g^2.
0	1	89	45	7,64	15,28
0	2	89	30	7,94	15,88
0	3	89	15	8,12	16,24
0	4	89	0	8,24	16,48
0	5	88	45	8,34	16,68
0	6	88	30	8,42	16,84
0	7	88	15	8,48	16,96
0	8	88	0	8,54	17,08
0	9	87	45	8,59	17,18
0	10	87	30	8,64	17,28
0	11	87	15	8,68	17,36
0	12	87	0	8,72	17,44
0	13	86	45	8,75	17,50
0	14	86	30	8,78	17,56
0	15	86	15	8,81	17,62
0	16	86	0	8,84	17,68
0	17	85	45	8,87	17,74
0	18	85	30	8,89	17,78
0	19	85	15	8,92	17,84
0	20	85	0	8,94	17,88
0	22	84	30	8,98	17,96
0	24	84	0	9,02	18,04
0	26	83	30	9,05	18,10
0	28	83	0	9,08	18,16
0	30	82	30	9,11	18,22
0	32	82	0	9,14	18,28
0	34	81	30	9,17	18,34

TAB. IV.

	10°	20°	30°	35°	40°	45°	50°	55°	60°
8,64	,64	,61	,57	,54	,50	,46	,42	,38	,33
8,68	,70	,67	,63	,59	,55	,51	,46	,41	,36
8,72	,77	,74	,68	,64	,60	,55	,50	,45	,39
8,75	,84	,80	,74	,70	,65	,60	,55	,49	,43
8,78	,90	,86	,79	,75	,70	,65	,59	,53	,46
8,81	,97	,92	,85	,81	,75	,69	,63	,56	,49
8,84	1, 0	,98	,91	,86	,80	,74	,67	,60	,52
8,87	1, 1	1, 0	,96	,91	,85	,79	,71	,64	,56
8,89	1, 2	1, 1	1, 0	,97	,90	,83	,76	,68	,59
8,92	1, 2	1, 2	1, 1	1, 0	,95	,88	,80	,71	,62
8,94	1, 3	1, 2	1, 1	1, 1	1, 0	,93	,84	,75	,66
8,98	1, 4	1, 3	1, 2	1, 2	1, 1	1, 0	,93	,83	,72
9,02	1, 5	1, 5	1, 4	1, 3	1, 2	1, 1	1, 0	,90	,79
9,05	1, 7	1, 6	1, 5	1, 4	1, 3	1, 2	1, 1	,98	,86
9,09	1, 8	1, 7	1, 6	1, 5	1, 4	1, 3	1, 2	1, 1	,92

TAB. V.

	10°	20°	30°	35°	40°	45°	50°	55°	60°
	89°	89°	89°	89°	89°	89°	89°	89°	89°
6,463	59′ 30″	59′ 32″	59′ 34″	59′ 36″	59′ 38″	59′ 40″	59′ 42″	59′ 44″	59′ 46″
6,765	— 0	— 4	— 8	— 11	— 14	— 18	— 21	— 26	— 30
6,941	58 30	58 35	58 42	58 46	58 51	58 56	— 2	— 8	— 15
7,066	— 0	— 7	— 14	— 22	— 28	— 35	58 43	58 51	58 59
7,163	57 30	57 38	57 49	57 57	— 5	— 14	— 23	— 33	— 45
7,242	— 0	— 11	— 24	— 33	57 42	57 53	— 4	— 17	— 30
7,309	56 30	56 42	56 58	— 8	— 19	— 32	57 45	57 59	— 12
7,367	— 0	— 14	— 32	56 43	56 56	— 10	— 25	— 42	— 0
7,418	55 30	55 46	— 6	— 19	— 34	56 50	— 6	— 25	57 45
7,464	— 0	— 18	55 40	55 54	— 10	— 28	56 47	— 7	— 30
7,505	54 30	54 49	— 14	— 39	55 47	— 7	— 28	56 48	— 15
7,543	— 0	— 21	54 48	— 5	— 14	55 45	— 8	— 33	56 59
7,578	53 30	53 50	— 22	54 40	— 1	— 14	55 49	— 16	— 45
7,610	— 0	— 25	53 56	— 16	54 38	— 3	— 30	55 59	— 29
7,640	52 30	52 57	— 30	53 51	— 15	54 42	— 11	— 41	— 15

EXPLANATION OF THE TABLES.

Tab. I. This table contains two arguments. One is the distance of the sun from the meridian, which is expressed in hours, minutes, and seconds; and also in terms of the log. cosine of the hour-angle. This argument is placed in the first column. The second stands at the top of the table, and consists of the minutes and seconds contained in the error of time. The table itself exhibits the logarithm of the area *g b* corresponding to any given error at any particular observation. But it was necessary to give the log. value of this area in both directions from the tangent *g d*, i. e. both when the computed is greater, and when it is less, than the real interval of time. These two values are included in the same column, but are separated from

each other by a point. The first quantity expresses the log. area in the latter, the second expresses it in the former case. Thus, for instance, in the fifth column we have 136,108, and we are to understand that the log. of the area gb is equal to 136, when the computed time is too small; and equal to 108, when the computed time is too great.

The argument at the top is carried to every twelve seconds of time, or every three minutes of a great circle; and should be continued as far as four or five minutes of time for the first twelve or fourteen degrees from the meridian; as far as three minutes for the next ten degrees; two minutes for the next ten degrees; and so on, (the time being diminished as the tangent of the hour-angle increases,) as far as sixty or sixty-five degrees, beyond which the table need not be extended. The argument in the first column should be carried to every eight seconds of time for the first twelve or fourteen degrees; and to every twenty seconds for the rest of the table.

Tab. II. This table consists of two columns. The first contains the differences betwixt the logs. of the area gb and gc, when the latit. is varied one minute in calculating the incremental area. These differences are negative as far as the fourteenth, because so far the area gc exceeds the area gb. In the second column are exhibited the errors in the assumed latit. corresponding to these differences.

Tab. III. This table is composed of four columns. The first contains the computed distance in time from noon; the second the altitude of the sun; the third the log. sine of the hour-angle, or the log. cosine of the altitude, expressed in the first column; and the fourth twice the logarithm in the former

column, being intended to save us the trouble of multiplying by 2, when we are finding the logarithm of $\frac{\varepsilon^2}{a}$.

Tab. IV. In this table there are two arguments. One of them occupies the first column, and consists of the log. sines of the azimuth, which are found by subtracting the log. cosine of the altitude from the log. sine of the hour-angle, as they stand in the preceding table. The other argument is placed at the top of the columns, and is formed of the assumed latitude. The table is designed to exhibit the product of the tangent of the azimuth multiplied into the cosine of the latit. and into 15; so that, by taking out this quantity, and multiplying it into the minutes contained in the error of time, we may at once determine how much the assumed latit. is to be varied in computing the incremental area.

Tab. V. There are two arguments in this table likewise. That which stands in the first column is the log. value of $\frac{\varepsilon^2}{a}$ obtained from the second table by subtracting the log. cosine of the altitude from twice the log. sine of the hour-angle. The second argument, which is placed above the columns, is the supposed latitude. The table itself contains the complement of the meridian altitude at the place of observation.

VIII. *A Fourth Catalogue of the comparative Brightness of the Stars.* By William Herschel, *LL. D. F. R. S.*

Read February 21, 1799.

Lustre of the stars in Auriga.			
1	*f*	5	2, 1 4, 1
2	*g*	5.6	4, 2, 1 2.4
3	*i*	4	3 – 37 5 Arietis – 3 – 44 Persei 3.44 Persei
4	*ω*	5	4, 2 2.4, 1
5		6	5 – 6
6		6	5 – 6 6 – 12
7	*ε*	4	7 – 10 7 –, 10
8	*ζ*	4	10, 8 33, 8 10 =, 8 –, 30
9		6.5	7 Camelop ; 9 11 Camelop, 9 –, 12 Camelop
10	*η*	4	7 – 10, 8 10 ; 33 7 –, 10 =, 8
11	*μ*	5	15; 11 – 20 15 ; 11 ; 21
12		6	6 – 12
13	*α*	1	13 – – 3 Lyræ
14		5	16, 14. 19
15	*λ*	5	15; 11 15 ; 11
16		6	16, 14
17		7.6	19. 17, 18
18		8	17, 18
19		6	14. 19. 17
20	*ρ*	6	11 – 20 21. 20
21	*σ*	5.6	11 ; 21. 20
22		6	26, 22

Lustre of the stars in Auriga.			
23	γ	2	13 Arietis, 23 . 34 34; 23 23, 34
24	φ	5.6	25 ; 24
25	χ	5.6	25 ; 26 25 ; 24
26		6	25 ; 26, 22 27, 26
27	o	6	27, 26
28		7	40 – 28
29	τ	5	32, 29, 31 35 . 29 –, 36
30	ξ	6	8 –, 30 42 Camel . 30 – 31 Camel
31	υ	6	29, 31 . 40
32	ν	5	32, 29
33	δ	4	10 ; 33, 8 33; 10 Camel
34	β	2	23 . 34 66 Gemin . 34 34; 23 23, 34
35	π	6	35 . 29 35, 46
36		6	29 –, 36
37	θ	4	3 – 37 24 Gemin, 37
38		6.7	39 . 38, 42
39		6.7	39 . 38
40		6	31 . 40 – 28
41		6	46 – 41
42		6	38, 42 . 43 47, 42 . 43
43		6	42 . 43
44	$\varkappa$	4.5	44, 136 Tauri 44 – – 49
45		6	45 . 46
46		5	35, 46 – 41 45 . 46
47		6	47, 42
48		6	49 –, 48 – 53
49		5.6	44 – – 49 –, 48
50		5.6	50 – 52 50 . 16 Lyncis
51		5.6	52 – 51
52		5	50 – 52 – 51
53		6	53 . 28 Gemin 48 – 53, 54
54		6	28 Gemin, 54 – 25 Gemin 53, 54

Lustre of the stars in Auriga.			
55		5	55 – 58
56		6	58 , 56 . 57 16 Lyncis , 56
57		6	56 . 57
58		4.5	55 – 58 , 56
59		6	62 . 59 . 60
60		6	59 . 60
61		6	61 . 62
62		6.7	61 . 62 . 59
63		4.5	63 . 65
64		5	66 ; 64
65		5	63 . 65 , 66
66		5	65 , 66 ; 64
Lustre of the stars in Draco.			
1	λ	3.4	1 . 5
2		6	5 – – 2 3 ; 2
3		6	3 ; 2
4		6	6 ; 4
5	ϰ	3	1 . 5 – – 2 11 – 5 , 13
6		6	6 ; 4
7		6	9 , 7
8		6	10 – 8 , 9
9		6	8 , 9 , 7
10	ι	5	10 – 8
11	α	2	11 – 5
12	ι	3	40 Herculis – 12 22 –, 12 27 Herc – 12 , 67 Herculis
13	θ	3	5 , 13
14	η	3	1 Ursæ min – 14 14 – – 22 14 – 28 14 –, 57
15	A	4	15 . 18
16		5	17 – 16

Lustre of the stars in Draco.			
17		5	17 – 16
18	g	5	19, 18 –, 20 15 . 18 21 . 18
19		5	19, 18
20	b	6	18 –, 20
21	μ	5.4	21 . 18 30, 21
22	ζ	2	14 – – 22 –, 12 57 –, 22
23	β	2.3	23, 40 Herculis 14 – 23 23 – 27 Herculis
24	ν'	4	24 . 25
25	ν^2	4	24 . 25 – 26
26		6	25 – 26
27	f	5	28 – 27, 34 27 . 42
28	ω	4	31 – 28 – 27
29		6	34 – –, 29
30		6	30, 21
31	ψ'	7	31 – 28
32	ξ	3	44 –, 32 – 43
33	γ	2	7 Ursæ min – 33 5 Coronæ –, 33 55 Ophiu – 33
34	ψ^2	4.5	27, 34 35 . 34 – –, 29 34 – 37
35		6	35 . 34 35 – 40 + 41
36		6	42 . 36
37		6	34 – 37, 38
38		6	37, 38
39	b	5	45, 39 . 46
40		5	41 – 40
41		5	41 – 40
42		6	27 . 42 . 36
43	φ	5	32 – 43
44	χ	4	44 –, 32
45	d	5	45, 39
46	c	5	39 . 46; 47

Lustre of the stars in Draco.			
47	o	4	46 ; 47 – – 51 47 – 54
48		6	49 . 48
49		6	51 . 49 . 48
50		4.5	52 . 50 ; 59 73 , 50 55 , 50 52 – 50
51		5.6	47 – – 51 . 49
52	υ	4.5	60 – 52 52 . 50 52 – 50
53		5	54 , 53
54		5	47 – 54 , 53
55		6	61 –, 55 55 , 50
56		6	Does not exist.
57	δ	3.4	14 –, 57 ; 22
58	π	4	63 –, 58 – 61 58 ; 67
59		6	50 ; 59
60	τ	4.5	60 – 52
61	σ	4.5	58 – 61 –, 55 67 , 61
62		6	Does not exist.
63	ε	5.6	63 –, 58
64	ε'	5.6	64 –, 65
65	ε'	6.5	64 –, 65 –, 70 65 , 69
66		6	68 . 66 . 71
67	ϱ	5	58 ; 67 , 61
68		6	68 . 66
69		6	65 , 69
70		6	65 –, 70
71		6	66 . 71
72		6	Does not exist.
73		5.6	73 , 50 73 – 77 73 , 78
74		6	75 – 74 . 76
75		6	75 – 74
76		5	74 . 76
77		5	73 – 77 78 , 77

		Lustre of the stars in Draco.	
78		5	73 , 78 , 77 16 Cephei , 78
79		7	80 – 79
80		6	80 – 79
		Lustre of the stars in Lynx.	
1		5.6	1 , 5
2		4	2 – – 5
3		6	8 – 3 , 10
4		6	4 . 6
5		6	2 – – 5 – 6 1 , 5
6		6.7	5 – 6 4 . 6
7		6.7	Does not exist.
8		6.7	8 , 41 Camelop 8 – 3
9		7	11 – 9
10		6.7	41 Camelop – 10 3 , 10
11		6	14 , 11 – 9
12		7	15 ; 12 –, 14
13		6	13 , 14
14		5	12 –, 14 13 , 14 , 11 19 , 14 – 23
15		5	15 ; 12
16		6	50 Aurigæ . 16 , 56 Aurigæ
17		7	18 –, 17
18		6	18 –, 47 Camelop 18 –, 17
19		5	24 – 19 , 14
20		6	21 – – 20
21		5	22 – 21 – – 20
22		6	22 – 21
23		7	14 – 23
24		5	24 – 19
25		6	26 –, 25
26		5	26 –, 25 26 , 34

Lustre of the stars in Lynx.			
27		5	27 – 50 Camelop 31 –, 27
28		7	36 – 28
29		5	29 – 56 Camelop 29, 58 Camelop 29, 30
30		6	29, 30
31		5	31 –, 27
32		7	33; 32
33		6	33; 32
34		6	26, 34 36, 34
35		7	Does not exist
36		6	36, 34 37 . 36 – 28
37		5.6	37 . 36 37, 42
38		6	38 – 40
39		4	12 Ursæ, 39 39 –, 10 Leonis min 39, 10 Ursæ
40		6	38 – 40
41		4	25 Ursæ . 41 – 12 Ursæ
42		6	37, 42 . 45 15 Leonis min . 42 – – 14 Leonis min
43		6	43 – 44
44		7.6	43 – 44
45		5.6	29 Ursæ – – 45 42 . 45
Lustre of the stars in Lyra.			
1	$\varkappa$	5	1 –, 2 6 – 1
2	μ	6	1 –, 2
3	α	1	16 Bootis – – 3
4	ε	5	6; 4 . 5
5		6	4 . 5
6	ζ	5	6 – – 7 6 – 1 6; 4
7		5	6 – – 7
8	ν^1	6	9, 8
9	ν^2	6	11 . 9, 8 15, 9, 8

			Lustre of the stars in Lyra.
10	β	3	10 . 14 14 ; 10 14 –, 10 14 – – – 10 6 + 7 ; 10
11	δ^2	4.5	12 –, 11 18, 11 . 9 16, 11
12	δ^1	4	12 –, 11 12 . 13 12 – 18 12 – 16
13	π	6	12 . 13 20, 13
14	γ	3	6 Cygni –, 14, 85 Herculis
15	λ	6	15, 9 15 . 17 15 ; 19
16	ρ	6	12 – 16, 11
17		6	15 . 17
18	ι	5	12 – 18, 11
19		6	15 ; 19
20	η	6	21 . 20, 13 20, 21
21	θ	6	21 . 20 20, 21
			Lustre of the stars in Monoceros.
1		6	3 –, 1 –, 2
2		6	1 –, 2, 4
3		6	3 –, 1
4		6	2, 4 . 6
5		4.5	11 – 5 –, 10 5, 8
6		6.7	4 . 6
7		6	10 – 7
8		4	5, 8 – 13 8 ; 15 8, 18
9		5	10 –, 9
10		6	5 –, 10 –, 9 10 – 7
11		5	22 Orionis – 11 – 5 30 ; 11 ; 26
12		5	13 –, 12, 14
13		4	8 – 13 = ; 14 13 –, 12
14		5.6	13 = ; 14 12, 14
15		4	8 ; 15, 17
16		6	17 – 16

Lustre of the stars in Monoceros.			
17		5	15, 17 – 16
18		4	8, 18
19		5	22 – 19, 20
20		6	19, 20, 25
21		5	22 –, 21, 24
22		4.5	22 –, 21 22 – 19
23		6.7	24, 23
24		6	21, 24, 23
25		6	20, 25
26		4.5	11 ; 26 ; 29
27		5	29 – 27 28, 27
28		5	29, 28, 27
29		6	29 – 27 26 ; 29, 28 29, 31
30		6	30; 11
31		4	29, 31
Lustre of the stars in Perseus.			
1		6	4, 1 – 3 4 – 1
2	*g*	6	3, 2
3		6.7	1 – 3, 2
4		6	4, 1 4 – 1 4 – 9
5		6	9 – 5 . 7 7 . 5
6	*b*	6	65 Androm, 6, 63 Androm
7	χ	7.6	5 . 7 . 8 8; 7 . 5
8		7	7 . 8 9 –, 8; 7
9	*i*	6	4 – 9 –, 8 9 – 5
10		7	Does not exist
11		7	27; 11 – 13
12	*q*	6	28 . 12 . 14
13	θ	4	11 – 13, 18 18 ; 13
14		6	12 . 14 24, 14

Lustre of the stars in Perseus.			
15		6	Lost
16	p^1	4	16, 22 16--20
17	r	5.6	17, 28
18	τ	5	13, 18 18, 13
19		6	Does not exist
20	p^2	6	16--20 28-20 43, 20
21		4.5	28. 21
22	π	4	16, 22, 28
23	γ	3	23, 25 23-4 Trianguli
24	s	6	28. 24, 14
25	ρ	4	23, 25-41 39-25
26	β	2.3	26, 25 26; 25 26---25 6 Arietis, 26-23
27	κ	5.4	27; 11
28	ω	5	22, 28. 12 17, 28. 24 28. 21 28-20
29		6	34-29. 31
30		6	32-30 36-30
31		5.6	29. 31
32	l	6	32-30 32-36
33	α	2.3	33--26 21 Androm, 33. 43 Andromedæ
34		6	35-34 34-29
35	σ	5	37, 35-34
36		6.7	32-36-30
37	ψ	5	37, 35
38	o^1	6	46, 38, 59 41-38-46
39	δ	3	45--39-25
40	o^2	6	52, 40. 42 40, 42
41	ν	4	25-41, 46 41-38
42	π	6	40. 42 40, 42 54; 42-55
43	A	5	43, 20
44	ζ	3	3 Aurigæ-44 3 Aurigæ. 44 44, 45
45	ι	3	44, 45-39

Lustre of the stars in Perseus.			
46	ξ	5	41, 46, 38 38-46 46-58
47	λ	4	51 · 47, 53
48	*c*	5	48, 51
49		6.7	50, 49
50		6.7	52, 50, 49
51	μ	4	48, 51 · 47
52	*f*	5	52, 50 53 · 52, 40 58 - 52
53	*d*	6	47, 53 · 52
54		6	54: 42
55		6	42 - 55 : 56
56		7	55: 56
57	*m*	6	59 · 57
58	*e*	5	46 - 58 - 52
59		6	38, 59 · 57

Lustre of the stars in Sextans.			
1		5	1 - 2
2		5	1 - 2
3		6	8 -, 3, 5 6, 3
4		6	7, 4 4 - 12
5		6	3, 5
6		6	8 - 6, 3
7		6	7, 4
8		6	8 -, 3 8 - 6
9		6	12, 9 · 13
10		6	29 Leonis = ♍ 10 - 11
11		5.6	10 - 11
12		6	4 - 12, 9
13		6	9 · 13
14		6	14 · 19
15		4	35 Hydræ, 15 - 32 Hydræ

Lustre of the stars in Sextans.			
16		6	19, 16
17		6	22, 17. 18
18		6	17. 18 – 20
19		6	14. 19, 16
20		6	18 – 20, 21
21		6	20, 21
22		6	22, 17
23		5	23, 26
24		6	28. 24 –, 26 33, 24
25		6	25. 26
26		6	24 –, 26 25. 26 23, 26 - 31
27		6	27, 28
28		5	29 – 28. 24 27, 28 – 32
29		5	30 – 29 – 28
30		5	32 Hydræ – 30 – 29
31		6	26 – 31
32		6	28 – 32
33		6	33, 24
34		6	35 – 34, 37
35		6	35 – 34
36		6	37. 36
37		6	55 Leonis –, 37, 38 34. 37. 36
38		6	37, 38
39		7	41 –, 39 40, 39
40		6	41 – 40, 39
41		6	41 –, 39 41 – 40
Lustre of the stars in Taurus.			
1	*o*	4	1 – 2
2	*ζ*	4	1 – 2, 35
3		6	Does not exist

Lustre of the stars in Taurus.			
4	*s*	6	5–, 4, 6 30–4
5	*f*	5	38–5–, 4 5–30
6	*t*	6	4, 6, 12
7		6	7. 66 Arietis
8		6	Does not exist
9		6	Lost
10		4.5	38, 10, 49
11		6	21. 11, 22
12		6	6, 12
13		6	13, 14 13–14
14		6	13, 14 13–14
15	*n*	6	Does not exist
16	*g*	7	18. 16. 21
17	*b*	5	27. 17. 20
18	*m*	7	28, 18. 16
19	*e*	5	20, 19. 23
20	*c*	6	17. 20, 19
21	*k*	6.7	16. 21. 11
22	*l*	7	11, 22, 26
23	*d*	5	19. 23–, 28
24	*p*	7	26, 24
25	*η*	3	27–, 27
26	*s*	7, 8	22, 26, 24
27	*f*	6	25–, 27. 17
28	*b*	7. 8	23–, 28, 18
29	u^1	6	29, 40
30	*e*	5	5–30–4 66, 30, 46
31	u^2	6	40, 31
32		6	53, 32. 33
33		7	32. 33
34		7	39–34
35	λ	4	2, 35. 38 123, 35

Lustre of the stars in Taurus.			
36		7	43 ; 36
37	A	5	65 , 37 -- 39 37 -- 43
38	ν	4	35 . 38 - 5 38 , 10
39		6	37 -- 39 51 - 39 43 - 39 - 34
40		7	29 , 40 , 31 46 ; 40 , 45
41		6	42 ; 41 ; 44
42	ψ	5	52 . 42 ; 41
43	ω^1	6	37 -- 43 - 39 43 ; 36
44	p	6	41 ; 44 , 59
45		7	40 , 45
46		7	47 , 46 ; 40 30 , 46 . 93
47		7	49 -, 47 , 46 47 . 60
48		7	58 . 48
49	μ	4	10 , 49 49 -, 47 88 , 49
50	ω^2	6	65 - 50 - 56 50 , 67
51		7	53 , 51 - 39
52	φ	5	52 . 42
53		7	56 , 53 , 51 53 , 32
54	γ	8	77 ; 54 -- 58 54 ; 61 74 ; 54
55		7	63 , 55
56		7	50 - 56 , 53
57		6 . 7	58 , 57 . 60
58	b	7	54 -- 58 . 48 58 , 57 73 - 58 -, 76 58 , 88
59	χ	5	44 , 59
60		7	57 . 60 47 . 60
61	δ^1	4	54 ; 61 , 68
62		7	72 - 62 65 ; 62
63		6	64 -, 63 , 55
64	δ^2	4	68 - 64 -, 63
65	$\varkappa$	5	65 - 69 65 ; 62 65 - 50 65 , 87
66	r	5	66 , 30
67	$\varkappa^2$	5	69 - 67 . 72 50 , 67 ; 72

Lustre of the stars in Taurus.			
68	δ^2	6	61, 68 – 64
69	υ^1	5	65 – 69 – 57 94 . 69
70		7	80 – 70
71		7	71; 75
72	υ^2	6	72 – 62 67 . 72 67; 72 72 –, 95
73	π	5	73 – 58 86, 73
74	ε	3 . 4	78, 74; 54
75		7	71; 75, 81
76		7	58 –, 76 83 – 76
77	θ^1	5	77 ; 54
78	θ^2	5	78, 74
79	b	5	90 – 79, 83
80		7	81, 80 - 70 80 – 84 80 . 85
81		7	75, 81, 80
82		7	Does not exist
83		7	79, 83 58, 83 – 76
84		7	80 - 84
85		7	80 . 85
86	ϱ	5	86, 78
87	α	1	58 Orionis – – 87 19 Orionis = ; 87 87 – – 78 Gemin
88	d	5	90, 88, 49
89		7	91; 89
90	c^1	5	90, 88 90 – –79 90 – – 93
91	σ^1	6	92, 91; 89
92	σ^2	6	92, 91
93	c^2	6	46 . 93 90 – – 93
94	τ	5	94 . 69
95		6 . 7	72 –, 95
96		6	4 Orionis – – 96 97 –, 96
97	i	6	4 Orionis, 97 –, 96
98	k	6	106 – 98 – 99 103 . 98

Lustre of the stars in Taurus.			
99		6	98 – 99, 107
100		6	Lost
101		6	107 ⁊ 101
102	ι	4	102 = ⁊ 106 102, 104
103		6	103 . 98
104	m	6	102, 104 –, 106
105		6	106, 105 – 107
106	l'	6	102 = ⁊ 106, 105 106 . 109 104 –, 106 – – 107 106 – 98
107	l^{2}	6	105 – 107 108 . 107 106 – – 107 99, 107 ⁊ 101
108		7	109 –, 108 109, 108 . 107
109	n	6	114 – 109 –, 108 106 . 109, 108
110		7	116 – 110 . 113 115 – 110 . 117 120 . 110
111		6.7	111, 116 111, 115 119 – 111
112	β	2	112 –, 24 Orionis
113		6	110 . 113
114	o	5	114 – 109
115		7.8	111, 115 – 110 122 . 115
116		6.7	111, 116 – 110
117		7	110 . 117
118		6	121 – 118
119		7	119 – 111
120		7	120 . 110
121		6	121 – 118
122		7	126 – 122 –, 129 122, 130 122 . 115
123	ζ	3	123 ⁊ 35 18 Gemin –, 123, 7 Gemin
124		6.7	Does not exist
125		3	132, 125
126		6	126 – 122
127		6	130 – – 127
128		6	129, 128
129		6	122 –, 129, 128

Lustre of the stars in Taurus.			
130		6	122, 130 – – 127
131		6	133, 131 . 132 135, 131, 137
132		4	139, 132, 125 131 . 132 . 135
133		6	134, 133, 131 134 – 133, 131
134		6	134, 133 134 – 133
135		6	132 . 135 – 138 135, 131
136		5	136 ⁞ 139 44 Aurigæ, 136
137		5	135 – 137 131, 137
138		6	Does not exist
139		6	1 Gemin – 139, 132 136 ⁞ 139, 132
140		6	141 . 140
141		6	141 . 140

Lustre of the stars in Triangulum.			
1	*d*	6	3 . 1
2	α	4	4 –, 2 –, 9 31 Androm –, 2 2 – 99 Piscium
3	*e*	6	7 – 3 . 1
4	β	4	6 Arietis – 4 23 Persei –, 4 –, 2 4 = 31 Andromedæ
5		7	7 – 5
6	*i*	6	6, 7 6, 10
7	η	6	8 – 7 – 3 6, 7 – 5 7; 14
8	δ	5	9 –, 8 – 7
9	γ	4	2 –, 9 –, 8
10	*a*	6	6, 10; 12
11	*d*	7	12, 11, 13
12	*c*	6	10; 12 ; 13 12, 11
13		7	12 ; 13 11, 13
14		6	7; 14; 15
15		7	14; 15
16		7	16, 30 Arietis 33 Arietis –, 16

Notes to Auriga.

23 Is 112 Tauri.

30 Is 32 Camelopardali.

45 "Oct. 5, 1798. The time of this star, in the observation "of FLAMSTEED, Vol. II. page 189, is marked :: but it cannot "be much out, as the star seems to be in the place assigned to "it by the British catalogue."

61. The RA in the *Atlas Cælestis* requires a correction of $-42'$.

Notes to Draco.

10 Is 87 Ursæ.

12 and 13 Were never observed by FLAMSTEED, but are in LA CAILLE's Catalogue of northern stars.

14 M. DE LA LANDE says the star is not to be found. See Mr. BODE's *Ast. Jahr-Buch* for 1795, page 198.

I observed this star in a sweep of the heavens, June 2d, 1788. Its brightness was estimated Sept. 11, 1795; Sept. 24, 1796; Sept. 30, 1796; and Dec. 28, 1798; so that, if M. DE LA LANDE is sure no cloud intervened when he looked for it, we may suspect it to be a changeable star.

15 The British catalogue requires $+30'$ in RA.

35 The expression "$35 - \overline{40 + 41}$" in my estimation of brightness, means that, with the naked eye, 35 is a very little brighter than 40 and 41 together, taken as one star. For they are so near each other, that the eye alone cannot distinguish them from a single star. The British catalogue gives them 3′ farther asunder than they ought to be according to FLAMSTEED's observation, page 463. See also Mr. BODE's *Ast. Jahr-Buch* for 1785, page 173.

40 The estimation " 40 — 41" was made with a 7-feet reflector, power 460.

56 Does not exist. FLAMSTEED has no observation of it. My double star II, 31, called 56 Draconis, is a star situated between 59 and 50, about 1¼ degree from 59 towards 50.*

62 Does not exist. FLAMSTEED has no observation of it; but, if an error of two hours be supposed in the calculation of one of the observations of 31 Draconis, it will account for the insertion of this star.

72 Does not exist. There is an observation, page 173, which produced it; but, if we admit an error of 3′ in time in that observation, it will then belong to 71.

Notes to Lynx.

7 Does not exist in the place pointed out by the British catalogue; but, in FLAMSTEED's observations, page 286, its time is marked : : and there is probably some considerable error.

20, 21, 22 The place of these stars in the heavens does not seem to agree with their situation in the Atlas.

* When I say 1¼ degree from 59 towards 50, it is to be understood that I express myself in degrees of a great circle. I have always used the same method of description in my catalogues of double stars; and, as these objects were pointed out for being viewed with telescopes of great magnifying power, which are generally not fixed, and therefore can give no right ascension, I am rather surprised to find that, in a catalogue published not many years ago, the author has taken my degrees of a great circle for degrees of right ascension. For instance, the double star IV, 82, where, in pointing out its place, I say, " above ½ degree following the 16 Cephei, in a line parallel to β " and α Cassiopeæ," is placed in the zone from 15 to 19° of that author's catalogue, only 2′ 47″,5 of time following 16 Cephei, when it ought to have been at least 10′ or 11′ following.

I take this opportunity to mention that, in general, the same author's account of my double stars is extremely erroneous.

30 Is 58 Camelopardali.

35. FLAMSTEED has no observation of this star; but, as it is marked 7m in the British catalogue, and has a line allotted to it, my Atlas and stars have been numbered so as to take it in; and the numbers I have used with double stars and other objects where the stars in Lynx after the 35th are concerned, must be reckoned accordingly.

37 "Dec. 4, 1796, This star is nearer to 25 than it is marked "in the Atlas." The RA should be corrected + 1ˢ.

Notes to Lyra.

10 This is one of our periodical stars discovered by Mr. GOODRICKE; its period is about 6 days 9 hours. See Phil. Trans. Vol. LXXVI. page 197. The greatest variation of its light, as far as I have observed, is from "10 . 14 to "$\overline{6+7}$ 10." The expression $\overline{6+7}$ is borrowed from algebra, and is always to be understood as has been explained in the note to 35 Draconis.

16 The British catalogue requires a correction of − 9° in PD; and this star will then agree with 12 Lyræ HEVELII.

19 The British catalogue requires a correction of + 8° in PD.

Notes to Perseus.

5 FLAMSTEED has no observation of this star; but there is a star exactly in the place pointed out by the British catalogue.

10 Does not exist. FLAMSTEED never observed it.

12 "Sept. 5, 1798, This star, which has no time in FLAM-"STEED's observations, is placed a little too forward; or requires "about + 10′ in RA."

14. "Sept. 4, 1798, The time of this star is marked doubtful "by FLAMSTEED, page 214; but it seems to be in the situation "where the British catalogue places it."

15 Is lost. FLAMSTEED observed it Jan. 17, 1693, page 186; but it is not to be seen in the place pointed out by that observation. See BODE's *Ast. Jahr-Buch* for 1794, page 97.

19 Does not exist. There is an observation in page 185, which has produced this star, but it belongs to 18; for the star is lettered r, and a memorandum says, "Post transitum." See also BODE's *Ast. Jahr-Buch* for 1788, page 172.

24. "Sept. 4, 1798. The place of this star in the British "catalogue wants a correction of + 56″ in PD, and — 45′ "in RA."

26 Is a periodical star. It has been noticed in the last century as subject to change, by MONTANARI and MARALDI; but its being periodical was discovered by Mr. GOODRICKE, in 1789, who fixed the time of its change at 2 days 20 hours 48′ 56″. See Phil. Trans. Vol. LXXIV. page 287. I have seen it when brightest, "6 Arietis, 26 – 23", and when most diminished, "26, 23".

38. "Sept. 5, 1798, The British catalogue requires nearly "+ 2° in RA, and — 13′ in PD; at least there is no other "star that can be taken for it."

42 "Sept. 4, 1798, The British catalogue requires a cor- "rection of + 18 in PD."

Notes to Sextans.

1 Is 10 Leonis.

10 Is 95 Leonis.

11 Is 28 Leonis.

12 "March 17, 1797, This star is misplaced in the British "catalogue; the PD should be corrected + 1°."

28 "March 21, 1797, This star is misplaced in the British "catalogue, and requires a correction of + 20′ in RA, and "+ 1° in PD."

29 "March 21, 1797, The PD of this star in the British "catalogue requires + 1°."

Notes to Taurus.

3 Does not exist. FLAMSTEED never observed it.

8 "Jan. 10, 1796, This star does not exist. FLAMSTEED "has no observation of it. There is a star about 9m not far "from the place."

9. "Dec. 28, 1798, This star is lost." M. DE LA LANDE says it is not to be found. See Mr. BODE's *Ast. Jahr-Buch* for 1795, page 198. FLAMSTEED has two complete observations of it, page 86, and page 566. We can hardly admit what Mr. BODE suggests, that this star, like the rest, has found its way into the British catalogue by some error of writing, or of calculating the observations; it will therefore be advisable to look for a future re-appearance of it, as it may prove to be a periodical or changeable one.

15 Does not exist. FLAMSTEED has no observation of it.

34 The estimation "39 – 34" belongs to a star very nearly in the place where, according to FLAMSTEED's observation, 34 should be; but, as we know by calculation that the Georgian planet was about the situation where, the 13 of Dec. 1690, FLAMSTEED observed the supposed 34th, there can be no doubt but that he must have seen it, and taken it for a fixed star.

The magnitude, 6m, which he assigned to 34, agrees perfectly well with the lustre of the planet, compared with other stars which the same author has marked 6m; and, with his telescope, he could not have the most distant suspicion of its being any other object than a fixed star of about the 6th magnitude.

40 " March 4, 1796. The RA in the Atlas requires a cor-" rection of about + 20′."

55 In the British catalogue, the PD requires — 8′.

56 The RA in the British catalogue requires — 15′.

82 Does not exist. FLAMSTEED did not observe this star, unless we admit a correction of the British catalogue — 1° 5′ in PD.

99 FLAMSTEED has no observation of this star; but, as there is one in the heavens, about a degree more north, the British catalogue requires probably a correction of — 1° in PD.

100 This star is lost. FLAMSTEED settled its place, page 369, and the observation seems to be a very good one.

103 FLAMSTEED has no observation of this star. How it came to be inserted in the British catalogue does not appear. I have given it as a double star V, 114, and here also estimated its brightness; but it must be remembered that my estimations do not strictly ascertain the place of objects. If, therefore, 103 does not exist, my double star, as well as the one here estimated, must be some star not far from the place assigned to 103 in the British catalogue.

112 Is 23 Aurigæ.

118 The Atlas should be corrected — 30′ in RA.

124 Does not exist; unless we admit a correction of + 1° 4′ in RA of the British catalogue.

138 Does not exist; but, as there is no time in FLAMSTEED's observation of this star, it is probably misplaced in the British catalogue, for there are several considerable stars in the neighbourhood.

Notes to Triangulum.

1 "Nov. 2, 1798." This star, which has the time and zenith distance in FLAMSTEED's observations doubtful, seems to be nearly in the place where the British catalogue gives it. It should perhaps be a few minutes more north.

Slough, near Windsor,
Jan. 28, 1799.

IX. *On a submarine Forest, on the east Coast of England.* By Joseph Correa de Serra, *LL.D. F.R.S. and A.S.*

Read February 28, 1799.

In geology, more perhaps than in any other branch of natural history, there exists a necessity of strictly separating the facts observed from the ideas which, in order to explain them, may occur to the mind of the observer. In the present state of this science, every well ascertained fact increases our still narrow stock of real knowledge; when, on the contrary, the reasonings we are enabled to make, are at best but ingenious guesses, which too often bias and mislead the judgment. I shall therefore endeavour, in this Paper, to give, first, a mere description of the object, unmixed with any systematical ideas, and shall afterwards offer such conjectures on its cause as seem to me to be fairly grounded on observation.

It was a common report in Lincolnshire, that a large extent of islets of moor, situated along its coast, and visible only in the lowest ebbs of the year, was chiefly composed of decayed trees. These islets are marked in Mitchell's chart of that coast, by the name of *clay buts*; and the village of Huttoft, opposite to which they principally lie, seems to have derived its name from them. In the month of September, 1796, I went to Sutton, on the coast of Lincolnshire, in company with the Right Hon. President of this Society, in order to examine their extent and nature. The 19th of the month, being the first day after the

equinoctial full moon, when the lowest ebbs were to be expected, we went in a boat, at half past twelve at noon, and soon after set foot upon one of the largest islets then appearing. Its exposed surface was about thirty yards long, and twenty-five wide, when the tide was at the lowest. A great number of similar islets were visible round us, chiefly to the eastward and southward; and the fishermen, whose authority on this point is very competent, say, that similar moors are to be found along the whole coast, from Skegness to Grimsby, particularly off Addlethorpe and Mablethorpe. The channels dividing the islets were, at the time we saw them, wide, and of various depths; the islets themselves ranging generally from east to west in their largest dimension.

We visited them again in the ebbs of the 20th and 21st; and, though it generally did not ebb so far as we expected, we could notwithstanding ascertain, that they consisted almost entirely of roots, trunks, branches, and leaves of trees and shrubs, intermixed with some leaves of aquatic plants. The remains of some of these trees were still standing on their roots; while the trunks of the greater part lay scattered on the ground, in every possible direction. The bark of the trees and roots appeared generally as fresh as when they were growing; in that of the birches particularly, of which a great quantity was found, even the thin silvery membranes of the outer skin were discernible. The timber of all kinds, on the contrary, was decomposed and soft, in the greatest part of the trees; in some, however, it was firm, especially in the knots. The people of the country have often found among them very sound pieces of timber, fit to be employed for several œconomical purposes.

The sorts of wood which are still distinguishable are birch,

fir, and oak. Other woods evidently exist in these islets, of some of which we found the leaves in the soil; but our present knowledge of the comparative anatomy of timbers, is not so far advanced as to afford us the means of pronouncing with confidence respecting their species. In general, the trunks, branches, and roots of the decayed trees, were considerably flattened; which is a phænomenon observed in the *Surtarbrand* or fossil wood of Iceland, and which SCHEUCHZER remarked also in the fossil wood found in the neighbourhood of the lake of Thun, in Switzerland.

The soil to which the trees are affixed, and in which they grew, is a soft greasy clay; but, for many inches above its surface, the soil is entirely composed of rotten leaves, scarcely distinguishable to the eye, many of which may be separated, by putting the soil in water, and dexterously and patiently using a spatula, or a blunt knife. By this method, I obtained some perfect leaves of *Ilex Aquifolium*, which are now in the Herbarium of the Right Hon. Sir JOSEPH BANKS; and some other leaves which, though less perfect, seem to belong to some species of willow. In this stratum of rotten leaves, we could also distinguish several roots of *Arundo Phragmites*.

These islets, according to the most accurate information, extend at least twelve miles in length, and about a mile in breadth, opposite to Sutton shore. The water without them, towards the sea, generally deepens suddenly, so as to form a steep bank. The channels between the several islets, when the islets are dry, in the lowest ebbs of the year, are from four to twelve feet deep; their bottoms are clay or sand, and their direction is generally from east to west.

A well dug at Sutton, by JOSHUA SEARBY, shows that a moor

of the same nature is found under ground, in that part of the country, at the depth of sixteen feet; consequently, very nearly on the same level with that which constitutes the islets. The disposition of the strata was found to be as follows:

Clay, - - - - -	16 feet.
Moor, similar to that of the islets,	from 3 to 4 ditto.
Soft moor, like the scowerings of a ditch bottom, mixed with shells and silt, -	20 ditto.
Marly clay, - - -	1 foot.
Chalk rock, - -	from 1 to 2 feet.
Clay, - - - - -	31 yards.
Gravel and water; the water has a chalybeate taste.	

In order to ascertain the course of this subterraneous stratum of decayed vegetables, Sir JOSEPH BANKS directed a boring to be made, in the fields belonging to the Royal Society, in the parish of Mablethorpe. Moor, of a similar nature to that of SEARBY's well, and of the islets, was found, very nearly on the same level, about four feet thick, and under it a soft clay.

The whole appearance of the rotten vegetables we observed, perfectly resembles, according to the remark of Sir JOSEPH BANKS, the moor which, in Blankeney fen, and in other parts of the East fen in Lincolnshire, is thrown up in the making of banks; barks, like those of the birch tree, being there also abundantly found. This moor extends over all the Lincolnshire fens, and has been traced as far as Peterborough, more than sixty miles to the south of Sutton. On the north side, the moory islets, according to the fishermen, extend as far as Grimsby, situated on the south side of the mouth of the Humber; and it is a remarkable circumstance, that in the large tracts of low lands which lie on the south banks of that river, a little

above its mouth, there is a subterraneous stratum of decayed trees and shrubs, exactly like those we observed at Sutton; particularly at Axholme isle, a tract of ten miles in length, by five in breadth; and at Hatfield chase, which comprehends one hundred and eighty thousand acres. DUGDALE* had long ago made this observation, in the first of these places; and DE LA PRYME† in the second. The roots are there likewise standing in the places where they grew; the trunks lie prostrate. The woods are of the same species as at Sutton. Roots of aquatic plants and reeds are likewise mixed with them; and they are covered by a stratum of some yards of soil, the thickness of which, though not ascertained with exactness by the above-mentioned observers, we may easily conceive to correspond with that which covers the stratum of decayed wood at Sutton, by the circumstance of the roots being (according to Mr. RICHARDSON's observations‡) only visible when the water is low, where a channel was cut, which has left them uncovered.

Little doubt can be entertained of the moory islets of Sutton being a part of this extensive subterraneous stratum, which, by some inroad of the sea, has been there stripped of its covering of soil. The identity of the levels; that of the species of trees; the roots of these affixed, in both, to the soil where they grew; and, above all, the flattened shape of the trunks, branches, and roots, found in the islets, (which can only be accounted for by the heavy pressure of a superinduced stratum,) are sufficient reasons for this opinion.

Such a wide spread assemblage of vegetable ruins, lying

* History of Embanking and Draining. Chap. xxvii.

† Philos. Trans. Vol. XXII. p. 980.

‡ Philos. Trans. Vol. XIX. p. 528.

almost in the same level, and that level generally under the common mark of low water, must naturally strike the observer, and give birth to the following questions.

1. What is the epoch of this destruction?

2. By what agency was it effected?

In answer to these questions, I will venture to submit the following reflections.

The fossil remains of vegetables hitherto dug up in so many parts of the globe, are, on a close inspection, found to belong to two very different states of our planet. The parts of vegetables, and their impressions, found in mountains of a cotaceous, schistous, or even sometimes of a calcareous nature, are chiefly of plants now existing between the tropics, which could neither have grown in the latitudes in which they are dug up, nor have been carried and deposited there by any of the acting forces under the present constitution of nature. The formation, indeed, of the very mountains in which they are buried, and the nature and disposition of the materials which compose them, are such as we cannot account for by any of the actions and re-actions which, in the actual state of things, take place on the surface of the earth. We must necessarily recur to that period in the history of our planet, when the surface of the ocean was at least so much above its present level, as to cover even the summits of these secondary mountains which contain the remains of tropical plants. The changes which these vegetables have suffered in their substance, is almost total; they commonly retain only the external configuration of what they originally were. Such is the state in which they have been found in England, by LLWYD; in France, by JUSSIEU; in the Netherlands, by BURTIN; not to mention in-

stances in more distant countries. Some of the impressions or remains of plants found in soils of this nature, which were, by more ancient and less enlightened oryctologists, supposed to belong to plants actually growing in temperate and cold climates, seem, on accurate investigation, to have been parts of exotic vegetables. In fact, whether we suppose them to have grown near the spot where they are found, or to have been carried thither from different parts, by the force of an impelling flood, it is equally difficult to conceive, how organized beings, which, in order to live, require such a vast difference in temperature and in seasons, could live on the same spot, or how their remains could (from climates so widely distant) be brought together to the same place, by one common dislocating cause. To this ancient order of fossil vegetables belong whatever retains a vegetable shape, found in or near coal mines, and (to judge from the places where they have been found) the greater part of the agatized woods. But, from the species and present state of the trees which are the subject of this Memoir, and from the situation and nature of the soil in which they are found, it seems very clear that they do not belong to this primeval order of vegetable ruins.

The second order of fossil vegetables, comprehends those which are found in strata of clay or sand; materials which are the result of slow depositions of the sea or of rivers, agents still at work under the present constitution of our planet. These vegetable remains are found in such flat countries as may be considered to be of a new formation. Their vegetable organization still subsists, at least in part; and their vegetable substance has suffered a change only in colour, smell, or con-

sistence; alterations which are produced by the development of their oily and bituminous parts, or by their natural progress towards rottenness. Such are the fossil vegetables found in Cornwall, by BORLASE; in Essex, by DERHAM; in Yorkshire, by DE LA PRYME, and RICHARDSON; and in foreign countries, by other naturalists. These vegetables are found at different depths, some of them much below the present level of the sea, but in clayey or sandy strata, (evidently belonging to modern formation,) and have, no doubt, been carried from their original place, and deposited there by the force of great rivers or currents, as it has been observed with respect to the Mississipi.* In many instances, however, these trees and shrubs are found standing on their roots, generally in low or marshy places, above, or very little below, the actual level of the sea.

To this last description of fossil vegetables, the decayed trees here described certainly belong. They have not been transported by currents or rivers; but, though standing in their native soil, we cannot suppose the level in which they are found, to be the same as that in which they grew. It would have been impossible for any of these trees and shrubs to vegetate so near the sea, and below the common level of its water: the waves would cover such tracts of land, and hinder any vegetation. We cannot conceive that the surface of the ocean has ever been lower than it now is; on the contrary, we are led by numberless phænomena to believe, that the level of the waters in our globe is much below what it was in former periods; we must therefore conclude, that the forest here de-

* LA COUDRENIÈRE *sur les Depots du Mississipi. Journ. de Phys.* Vol. XXI. p. 230.

scribed grew in a level high enough to permit its vegetation; and that the force (whatever it was) which destroyed it, lowered the level of the ground where it stood.

There is a force of subsidence (particularly in soft ground) which, being a natural consequence of gravity, slowly though perpetually operating, has its action sometimes quickened and rendered sudden by extraneous causes, for instance, by earthquakes. The slow effects of this force of subsidence have been accurately remarked in many places; examples also of its sudden action are recorded in almost every history of great earthquakes. The shores of Alexandria, according to Dolomieu's observations, are a foot lower than they were in the time of the Ptolemies. Donati, in his natural history of the Adriatic, has remarked, seemingly with great accuracy, the effects of this subsidence at Venice; at Pola, in Istria; at Lissa, Bua, Zara, and Diclo, on the coast of Dalmatia. In England, Borlase has given, in the Philosophical Transactions,* a curious observation of a subsidence, of at least sixteen feet, in the ground between Sampson and Trescaw islands, in Scilly. The soft and low ground between the towns of Thorne and Gowle, in Yorkshire, a space of many miles, has so much subsided in latter times, that some old men of Thorne affirmed, "that "whereas they could before see little of the steeple, (of Gowle,) "they now see the churchyard wall."† The instances of similar subsidence which might be mentioned, are innumerable.

This force of subsidence, suddenly acting by means of some earthquake, seems to me the most probable cause to which the actual submarine situation of the forest we are speaking

* Vol. XLVIII. p. 62.

† Gough's edition of Camden's Britannia, T. III. p. 35.

of may be ascribed. It affords a simple easy explanation of the matter; its probability is supported by numberless instances of similar events; and it is not liable to the strong objections which exist against the hypothesis of the alternate depression and elevation of the level of the ocean; an opinion which, to be credible, requires the support of a great number of proofs, less equivocal than those which have hitherto been urged in its favour, even by the genius of a LAVOISIER.*

The stratum of soil, sixteen feet thick, placed above the decayed trees, seems to remove the epoch of their sinking and destruction, far beyond the reach of any historical knowledge. In CÆSAR's time, the level of the North sea appears to have been the same as in our days. He mentions the separation of the Wahal branch of the Rhine, and its junction to the Meuse; noticing the then existing distance from that junction to the sea; which agrees, according to D'ANVILLE's inquiries,† with the actual distance. Some of the Roman roads constructed by order of AUGUSTUS, under AGRIPPA's administration, leading to the maritime towns of Belgium, still exist, and reach the present shore.‡ The descriptions which Roman authors have left us, of the coasts, ports, and mouths of rivers, on both sides of the North sea, agree in general with their present state; except in the places ravaged by the inroads of this sea, more apt, from its form, to destroy the surrounding countries, than to increase them.

An exact resemblance exists between maritime Flanders and

* *Mém. de l'Acad. de Paris.* 1789. p. 351.

† D'ANVILLE *Notice des Gaules.* p. 461.

‡ NICOL. BERGIER. *Hist. des grands Chemins des Romains. Ed. de Bruxelles.* Vol. II. p. 109.

the opposite low coast of England, both in point of elevation above the sea, and of internal structure and arrangement of their soils. On both sides, strata of clay, silt, and sand, (often mixed with decayed vegetables,) are found near the surface; and, in both, these superior materials cover a very deep stratum of bluish or dark-coloured clay, unmixed with extraneous bodies. On both sides, they are the lowermost part of the soil, existing between the ridges of high lands,* on their respective sides of the same narrow sea. These two countries are certainly coeval; and, whatever proves that maritime Flanders has been for many ages out of the sea, must, in my opinion, prove also, that the forest we are speaking of was long before that time destroyed, and buried under a stratum of soil. Now it seems proved, from historical records, carefully collected by several learned members of the Brussels Academy, that no material change has happened to the lowermost part of maritime Flanders, during the period of the last two thousand years.†

I am therefore inclined to suppose the original catastrophe which buried this forest, to be of a very ancient date; but I suspect the inroad of the sea which uncovered the decayed trees of the islets of Sutton, to be comparatively recent. The state of the leaves and of the timber, and also the tradition of

* These ridges of high lands, both on the British and Belgic side, must be very similar to each other, since they both contain parts of tropical plants in a fossil state. Cocoa nuts, and fruits of the areca, are found in the Belgic ridge. The petrified fruits of Sheppey, and other impressions of tropical plants, on this side of the water, are well known.

† Vide several papers in the Brussels *Mémoires*; also *Journ. de Phys.* T. XXXIV. p. 401.

the neighbouring people, concur to strengthen this suspicion. Leaves and other delicate parts of plants, though they may be long preserved in a subterraneous situation, cannot remain uninjured, when exposed to the action of the waves and of the air. The people of the country believe, that their parish church once stood on the spot where the islets now are, and was submerged by the inroads of the sea; that, at very low water, their ancestors could even discern its ruins; that their present church was built to supply the place of that which the waves washed away; and that even their present clock belonged to the old church. So many concomitant though weak testimonies, incline me to believe their report, and to suppose that some of the stormy inundations of the North sea, which in these last centuries have washed away such large tracts of land on its shores, took away a soil resting on clay, and at last uncovered the trees which are the subject of this Paper.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1798.

1798	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Jan. 1	38	8	0	39	54	29,67	86	0,240	W	1	Fair.
	45	2	0	45	55	29,67	81		W	1	Cloudy.
2	39	8	0	40	54	29,77	84		W	1	Fair.
	41	2	0	41	57	29,88	79		NW	1	Fine.
3	34	8	0	36	54	29,94	83		NW	1	Cloudy.
	40	2	0	39	54	29,97	83		E	1	Cloudy.
4	36	8	0	42	53	29,95	87		SSE	1	Cloudy.
	49	2	0	49	56	29,83	88		S	2	Cloudy.
5	41	8	0	41	54	29,71	82	0,092	SW	2	Cloudy.
	44	2	0	44	57	29,68	78		W	2	Fine.
6	37	8	0	41	53	29,46	84	0,022	W	2	Cloudy.
	43	2	0	42	55	29,83	78		NNE	2	Fair.
7	31	8	0	31	53	30,27	82		NNE	2	Fine.
	36	2	0	36	55	30,34	74		NE	2	Fine.
8	30	8	0	31	52	30,46	74		NE	2	Cloudy.
	33	2	0	32	53	30,48	73		NE	2	Cloudy.
9	31	8	0	32	50	30,46	74		NE	1	Cloudy.
	33	2	0	33	52	30,38	75		NE	1	Cloudy.
10	30	8	0	30	49	30,33	78		NE	1	Cloudy.
	32	2	0	32	51	30,30	76		NE	1	Cloudy.
11	28	8	0	32	49	30,15	79		SE	1	Cloudy.
	38	2	0	38	52	30,04	82		SE	1	Rain.
12	33	8	0	33	50	29,89	87	0,115	E	1	Snow.
	36	2	0	36	51	29,92	87		E	1	Cloudy.
13	33	8	0	33	50	30,11	85	0,033	NE	1	Cloudy.
	39	2	0	38	51	30,14	84		NW	1	Cloudy.
14	32	8	0	36	50	30,11	87		SW	1	Cloudy.
	47	2	0	47	51	29,98	80		W	1	Cloudy.
15	44	8	0	46	51	29,86	89		W	1	Cloudy.
	50	2	0	50	55	29,77	86		SW	1	Cloudy.
16	43	8	0	43	53	29,55	85	0,038	WSW	1	Cloudy.
	44	2	0	44	54	29,49	84		SW	1	Cloudy.

METEOROLOGICAL JOURNAL

for January, 1798.

1798	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Jan. 17	33	8	0	33	50	29,59	88	0,053			Foggy.
	47	2	0	44	55	29,26	87		E	1	Cloudy.
18	42	8	0	42	53	28,96	89	0,142	SE	1	Fine.
	48	2	0	47	56	29,02	82		SE	2	Cloudy.
19	42	8	0	42	54	29,60	87		NE	1	Cloudy.
	46	2	0	46	56	29,80	85		NE	1	Cloudy.
20	37	8	0	42	53	30,22	88		SW	1	Cloudy.
	49	2	0	49	55	30,31	89		SSW	1	Cloudy.
21	47	8	0	48	55	30,42	90		S	1	Cloudy.
	53	2	0	53	57	30,43	88		S	1	Cloudy.
22	48	8	0	48	55	30,15	87		SSE	2	Cloudy.
	51	2	0	51	56	30,05	88		SW	2	Cloudy.
23	35	8	0	35	54	30,48	82	0,020	WSW	1	Fair.
	43	2	0	43	59	30,52	77		NW	1	Fine.
24	35	8	0	40	53	30,28	82		W	2	Cloudy.
	48	2	0	46	57	30,10	83		SW	2	Cloudy.
25	37	8	0	38	54	30,21	80		NW	2	Fair.
	42	2	0	41	56	30,33	75		NE	2	Cloudy.
26	29	8	0	29	53	30,41	80		WSW	1	Fair.
	41	2	0	41	55	30,30	78		SSW	1	Cloudy.
27	38	8	0	42	53	30,02	88	0,025	SSW	1	Rain.
	47	2	0	47	56	30,05	82		N	1	Cloudy.
28	30	8	0	30	53	30,30	84		SW	1	Fine.
	40	2	0	40	56	30,24	79		S	2	Fine.
29	34	8	0	38	52	29,65	81		S	2	Cloudy.
	43	2	0	43	54	29,40	83		SSE	2	Cloudy.
30	32	8	0	32	51	29,49	87	0,150	SW	1	Fine.
	46	2	0	43	55	29,44	82		S	2	Cloudy.
31	40	8	0	40	51	29,10	87	0,175	S	2	Fine. [Much wind last night.
	44	2	0	41	54	29,16	84		WSW	2	Rain.

METEOROLOGICAL JOURNAL
for February, 1798.

1798	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Feb. 1	33	7 0	33	48	29,80	83	0,063	WSW	2	Fine.
	45	2 0	44	55	29,91	79		W	2	Fine.
2	39	7 0	41	53	29,98	88	0,224	SE	1	Rain.
	53	2 0	53	56	29,93	89		WSW	1	Cloudy.
3	47	7 0	47	53	29,99	89		SW	2	Cloudy.
	54	2 0	54	57	30,01	86		WSW	2	Cloudy.
4	39	7 0	39	55	30,02	82		WSW	1	Fair.
	45	2 0	45	58	30,33	81		NE	1	Fair.
5	34	7 0	35	55	30,59	83		ENE	1	Cloudy.
	42	2 0	42	56	30,60	77		E	1	Cloudy.
6	31	7 0	31	53	30,58	86		E	1	Cloudy.
	39	2 0	39	56	30,60	82		NE	1	Foggy.
7	31	7 0	31	53	30,73	85		NE	1	Foggy.
	40	2 0	40	55	30,76	82		SE	1	Foggy.
8	28	7 0	29	53	30,68	83		SW	1	Fair.
	41	2 0	41	56	30,66	75		W	1	Fair.
9	32	7 0	33	54	30,51	84		SW	1	Cloudy.
	42	2 0	42	55	30,45	81		WNW	1	Fair.
10	41	7 0	43	55	30,39	87		SW	1	Cloudy.
	48	2 0	48	57	30,37	82		WNW	1	Cloudy.
11	38	7 0	38	55	30,38	87		SW	1	Fine.
	50	2 0	50	57	30,42	86		WNW	1	Cloudy.
12	38	7 0	40	55	30,47	85		SW	1	Cloudy.
	46	2 0	46	57	30,46	79		WSW	1	Cloudy.
13	42	7 0	42	54	30,40	81		SW	1	Cloudy.
	50	2 0	50	57	30,38	77		WSW	1	Cloudy.
14	41	7 0	43	54	30,34	86		SW	1	Cloudy.
	50	2 0	50	57	30,23	87		SW	2	Cloudy.
15	50	7 0	50	57	29,96	88		SSW	2	Cloudy.
	54	2 0	54	58	29,87	89		SSW	2	Rain.
16	34	7 0	34	56	29,98	81	0,120	WNW	2	Fine.
	43	2 0	43	57	29,96	74		NW	2	Fine.

METEOROLOGICAL JOURNAL
for February, 1798.

1798	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Feb. 17	31	7	0	32	55	29,95	82		NE	1	Cloudy.
	37	2	0	37	54	29,95	76		NE	1	Cloudy.
18	26	7	0	26	52	30,12	82		NE	1	Fair.
	38	2	0	37	55	30,10	78		NE	2	Fair.
19	24	7	0	24	50	29,94	80		NE	1	Fine.
	34	2	0	34	53	29,85	79		NW	2	Fair.
20	25	7	0	25	49	29,60	80		E	1	Fine.
	38	2	0	38	53	29,59	75		SSE	1	Fair.
21	33	7	0	33	50	29,70	86		W	1	Cloudy.
	43	2	0	43	53	29,69	75		S	1	Fair.
22	35	7	0	35	50	29,23	87	0,058	SSW	2	Fine.
	45	2	0	45	54	29,38	71		W	2	Fine.
23	33	7	0	33	51	29,77	83		SW	1	Cloudy.
	47	2	0	45	53	29,72	84		S	1	Rain.
24	45	7	0	48	53	29,54	90	0,080	S	2	Rain.
	49	2	0	48	55	29,60	90		NW	2	Cloudy.
25	36	7	0	36	53	29,64	84	0,148	SW	1	Fine.
	46	2	0	46	56	29,70	77		SW	2	Fine.
26	34	7	0	34	53	29,86	86		SW	2	Fine.
	46	2	0	45	55	30,02	77		NW	1	Fair.
27	31	7	0	31	53	30,28	83		SW	1	Fair.
	43	2	0	43	57	30,32	74		NNW	1	Fair.
28	32	7	0	32	53	30,34	84		E	1	Fair.
	46	2	0	46	56	30,27	76		ESE	1	Fine.

METEOROLOGICAL JOURNAL
for March, 1798.

1798	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
Mar. 1	31	7	0	31	53	30,20	83		NE	1	Fair.
	50	2	0	49	56	30,20	77		SSE	1	Fair.
2	38	7	0	38	54	30,21	85		E	1	Hazy.
	54	2	0	54	58	30,24	78		SSE	1	Fair.
3	46	7	0	46	56	30,34	90		SSE	1	Cloudy.
	58	2	0	57	58	30,34	77		SE	1	Fine.
4	46	7	0	46	57	30,37	88		SW	1	Cloudy.
	55	2	0	54	59	30,37	85		SW	1	Cloudy.
5	49	7	0	49	58	30,27	89		SW	1	Cloudy.
	56	2	0	56	60	30,27	82		SW	1	Cloudy.
6	49	7	0	49	58	30,15	86		SW	1	Cloudy.
	56	2	0	56	60	30,16	83		NW	1	Cloudy.
7	47	7	0	47	59	30,18	76		E	1	Cloudy.
	54	2	0	54	59	30,18	76		ESE	1	Cloudy.
8	48	7	0	48	58	30,13	87	0,038	SSW	1	Cloudy.
	55	2	0	55	59	30,12	79		NW	1	Cloudy.
9	42	7	0	42	58	30,11	83		E	1	Cloudy.
	58	2	0	58	61	30,05	76		SSE	1	Cloudy.
10	44	7	0	44	59	29,88	84		SW	1	Cloudy.
	57	2	0	56	60	29,79	72		W	1	Cloudy.
11	34	7	0	34	58	29,82	82	0,150	ENE	1	Fair.
	41	2	0	41	60	29,88	72		N	1	Fine.
12	30	7	0	30	55	30,05	80		NE	1	Fine.
	42	2	0	42	57	30,04	72		E	1	Fair.
13	31	7	0	31	54	29,93	78		E	1	Fair.
	41	2	0	41	55	29,88	70		E	2	Fine.
14	31	7	0	31	54	30,06	80		NE	1	Fair.
	45	2	0	45	57	30,12	74		NE	1	Fair.
15	33	7	0	33	53	30,16	81		WSW	1	Fair.
	55	2	0	55	55	30,10	76		SW	1	Cloudy.
16	44	7	0	44	56	29,88	85		WSW	1	Cloudy.
	52	2	0	50	57	29,70	74		WNW	1	Fine.

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1798	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Mar. 17	36	7	0	38	54	29,40	82		WNW	1	Cloudy.
	48	2	0	48	56	29,27	73		WNW	2	Cloudy.
18	35	7	0	36	55	29,18	83	0,025	N	1	Cloudy.
	44	2	0	41	57	29,25	81		NE	1	Rain.
19	36	7	0	36	54	29,43	85	0,037	NE	1	Cloudy.
	43	2	0	42	56	29,49	82		NE	1	Cloudy.
20	30	7	0	30	53	29,55	84	0,058	NE	1	Cloudy.
	42	2	0	42	54	29,54	74		NE	1	Hazy.
21	30	7	0	31	52	29,70	84		NE	1	Fine.
	44	2	0	42	54	29,83	80		NE	1	Cloudy.
22	30	7	0	31	50	29,88	86		NE	1	Cloudy.
	43	2	0	43	54	29,90	76		NE	2	Cloudy.
23	31	7	0	33	51	30,08	81		NE	1	Cloudy.
	44	2	0	44	53	30,15	73		NE	1	Cloudy.
24	36	7	0	36	51	30,20	83		NE	1	Cloudy.
	43	2	0	43	53	30,13	75		NE	1	Cloudy.
25	39	7	0	40	53	30,00	84		E	1	Cloudy.
	48	2	0	48	54	29,97	77		E	1	Cloudy.
26	36	7	0	38	52	29,89	85		NE	1	Cloudy.
	49	2	0	47	54	29,83	75		NE	1	Fair.
27	35	7	0	36	52	29,94	83		NE	1	Cloudy.
	46	2	0	46	54	29,98	75		NE	1	Cloudy.
28	39	7	0	39	51	29,90	78		NW	1	Cloudy.
	48	2	0	48	54	29,80	81		N	1	Cloudy.
29	40	7	0	40	53	29,72	85		NE	1	Cloudy.
	47	2	0	46	55	29,72	81		NE	1	Cloudy.
30	36	7	0	36	53	29,58	81	0,025	E	1	Rain.
	44	2	0	44	56	29,61	76		E	1	Cloudy.
31	30	7	0	30	52	29,87	77		NE	1	Fair.
	40	2	0	40	56	29,91	68		NW	2	Fair.

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1798	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°										
April 1	30	7	0	31	53	30,00	77		W	1	Fair.
	46	2	0	45	56	30,00	70		NW	3	Fair.
2	36	7	0	40	53	29,87	79		SE	2	Cloudy.
	49	2	0	48	55	29,75	72		SE	2	Cloudy.
3	39	7	0	40	53	29,46	84		W	2	Fine.
	54	2	0	54	57	29,54	71		W	1	Cloudy.
4	47	7	0	51	56	29,27	89	0,335	S	2	Rain.
	55	2	0	54	57	29,30	83		S	2	Cloudy.
5	48	7	0	49	55	29,47	82	0,018	S	2	Cloudy.
	58	2	0	57	59	29,57	73		S	2	Fair.
6	48	7	0	48	55	29,65	78	0,023	S	2	Fair.
	58	2	0	56	60	29,86	75		S	2	Cloudy.
7	47	7	0	50	58	30,08	84	0,038	S	1	Fair.
	62	2	0	62	62	30,11	73		S	2	Fair.
8	46	7	0	50	60	30,26	83		S	1	Fine.
	66	2	0	64	62	30,31	72		S	1	Fair.
9	43	7	0	45	60	30,38	82		NE	1	Hazy.
	66	2	0	66	63	30,38	70		E	1	Fine.
10	46	7	0	48	62	30,33	83		E	1	Fine.
	67	2	0	67	64	30,29	70		E	2	Fine.
11	49	7	0	51	61	30,33	71		E	2	Fine.
	61	2	0	61	66	30,33	66		E	2	Fine.
12	45	7	0	48	60	30,26	68		E	2	Fine.
	58	2	0	58	63	30,19	66		E	2	Fine.
13	43	7	0	45	60	30,04	73		E	1	Fair.
	63	2	0	62	63	29,99	66		E	2	Fine.
14	47	7	0	50	61	29,95	76		E	2	Fine.
	60	2	0	58	63	29,89	75		SE	2	Cloudy.
15	49	7	0	51	61	29,88	80		SSW	2	Cloudy.
	60	2	0	60	62	29,88	72		S	2	Cloudy.
16	50	7	0	50	60	29,60	78	0,020	SSW	2	Cloudy.
	61	2	0	61	63	29,68	69		SSW	2	Fair.

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1798	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Apr. 17	47	7	0	49	61	29,72	77		ENE	1	Cloudy.
	59	2	0	59	62	29,68	74		E	1	Cloudy.
18	47	7	0	48	61	29,72	81		NE	1	Cloudy.
	56	2	0	56	62	29,75	74		NE	1	Cloudy.
19	45	7	0	46	60	29,70	80	0,083	E	1	Cloudy.
	56	2	0	54	61	29,60	76		SE	1	Cloudy.
20	43	7	0	45	60	29,88	*		NE	2	Fair.
	50	2	0	50	61	30,02			NE	2	Fair.
21	40	7	0	42	58	30,21			NE	1	Cloudy.
	52	2	0	52	58	30,21			NE	1	Cloudy.
22	38	7	0	41	57	30,23			ESE	1	Fine.
	55	2	0	55	60	30,18			E	1	Fine.
23	38	7	0	43	58	30,15			E	1	Fair.
	60	2	0	60	60	30,08			E	1	Fine.
24	43	7	0	48	58	30,12			NE	1	Hazy.
	65	2	0	65	59	30,14			E	1	Fine.
25	43	7	0	47	59	30,20			ENE	1	Fine.
	59	2	0	59	60	30,20			ESE	1	Fine.
26	44	7	0	47	59	30,20			E	1	Fair.
	67	2	0	67	60	30,11			SSE	1	Fine.
27	48	7	0	52	60	30,08			SSW	1	Hazy.
	69	2	0	69	62	30,03			SSE	1	Fine.
28	47	7	0	52	61	29,98			NE	1	Fair.
	66	2	0	65	64	29,94			E	1	Fine.
29	45	7	0	48	62	29,89			E	1	Cloudy.
	57	2	0	57	61	29,87			NE	1	Cloudy.
30	42	7	0	49	60	29,84			NE	1	Cloudy.
	58	2	0	57	60	29,81			E	1	Fair.

* The whalebone of the Hygrometer slipped out of its place.

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1798	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
May 1	45	7 0	46	59	29,85	52		NE	1	Cloudy.
	54	2 0	53	58	29,85	48		NE	1	Cloudy.
2	48	7 0	49	58	30,02	56		NE	1	Cloudy.
	58	2 0	57	58	30,08	45		NE	1	Cloudy.
3	43	7 0	48	58	30,23	53		NE	1	Fair.
	64	2 0	64	60	30,24	39		NE	1	Fine.
4	46	7 0	46	59	30,28	54		NE	1	Cloudy.
	59	2 0	59	59	30,21	45		NE	1	Fair.
5	47	7 0	50	59	30,10	54		E	1	Cloudy.
	58	2 0	58	59	30,10	48		SSE	1	Cloudy.
6	49	7 0	51	59	30,08	60		SW	1	Cloudy.
	63	2 0	60	60	30,09	44		N	1	Cloudy.
7	50	7 0	50	59	30,12	66	0,375	S	1	Rain.
	63	2 0	62	60	30,12	50		S	1	Cloudy.
8	46	7 0	47	59	30,13	69		SSW	1	Cloudy.
	66	2 0	66	61	30,09	45		S	1	Fine.
9	48	7 0	50	60	30,03	57	0,100	SW	1	Fine.
	67	2 0	66	60	29,97	50		SSW	1	Cloudy.
10	47	7 0	49	60	29,87	60		SSW	1	Fine.
	67	2 0	67	61	29,82	42		SW	1	Fair.
11	47	7 0	49	60	29,70	55		NW	1	Fine.
	63	2 0	57	59	29,68	56		SSW	1	Rain.
12	49	7 0	51	59	29,42	59	0,048	SSW	2	Fair.
	61	2 0	61	60	29,31	53		SSW	2	Cloudy.
13	46	7 0	52	59	29,51	60	0,250	WSW	1	Cloudy.
	63	2 0	62	60	29,50	48		WSW	1	Rain.
14	49	7 0	51	59	29,32	61	0,168	SSE	2	Fair.
	56	2 0	56	59	29,11	65		SSE	2	Rain.
15	51	7 0	52	58	29,50	64	0,165	SSE	1	Rain.
	68	2 0	67	61	29,49	53		SSE	2	Fair.
16	52	7 0	53	60	29,64	60	0,045	SSE	1	Rain.
	65	2 0	64	60	29,67	61		ENE	1	Cloudy.

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1798	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
May 17	53	7 0	54	60	29,79	63	0,265	S	2	Cloudy.
	64	2 0	64	61	29,96	45		S	2	Fair.
18	50	7 0	52	60	30,18	61		SW	1	Cloudy.
	66	2 0	65	62	30,18	38		WNW	1	Fair.
19	50	7 0	53	60	30,23	57		SW	1	Cloudy.
	64	2 0	62	60	30,15	47		SW	1	Cloudy.
20	52	7 0	54	60	30,16	53		SSW	1	Cloudy.
	62	2 0	62	61	30,24	38		NW	1	Cloudy.
21	46	7 0	51	60	30,42	52		WNW	1	Fine.
	66	2 0	66	61	30,44	43		NW	1	Fair.
22	47	7 0	50	60	30,44	56		ESE	1	Cloudy.
	65	2 0	65	61	30,41	38		ENE	1	Fine.
23	45	7 0	49	60	30,41	57		NE	1	Fine.
	64	2 0	64	61	30,38	40		E	1	Fine.
24	46	7 0	51	60	30,32	53		E	1	Fine.
	67	2 0	66	61	30,27	30		E	1	Hazy.
25	51	7 0	54	61	30,18	50		S	1	Hazy.
	73	2 0	72	63	30,14	37		E	1	Fine.
26	50	7 0	54	61	30,13	52		E	1	Fine.
	72	2 0	71	63	30,13	36		E	1	Fine.
27	53	7 0	56	63	30,20	60		E	1	Fine.
	76	2 0	75	64	30,16	34		E	1	Fine.
28	54	7 0	58	64	30,11	47		NE	1	Fine.
	73	2 0	73	66	30,01	32		NE	2	Fine.
29	50	7 0	54	64	30,09	47		NE	2	Cloudy.
	66	2 0	60	64	30,09	39		NE	2	Cloudy.
30	49	7 0	53	64	30,09	54		NE	2	Cloudy.
	60	2 0	54	64	30,05	60		N	1	Rain.
31	51	7 0	54	64	29,92	65	0,205	NE	1	Cloudy.
	61	2 0	61	63	29,87	62		NE	1	Cloudy.

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1798	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
June 1	53	7	0	54	63	29,80	69		NE	1	Cloudy.
	68	2	0	68	63	29,77	54		NE	1	Cloudy.
2	57	7	0	57	63	29,78	65		S	2	Cloudy.
	69	2	0	68	63	29,84	51		S	1	Cloudy.
3	48	7	0	51	63	30,10	58	0,035	S	1	Fair.
	70	2	0	69	63	30,13	36		SW	1	Cloudy.
4	57	7	0	58	62	30,18	61		S	2	Cloudy.
	73	2	0	71	63	30,23	49		SSW	2	Cloudy.
5	57	7	0	57	63	30,28	67		W	1	Cloudy.
	73	2	0	71	64	30,30	52		S	1	Hazy.
6	56	7	0	58	64	30,32	63		E	1	Cloudy.
	74	2	0	74	66	30,30	48		E	1	Fine.
7	59	7	0	63	66	30,26	67		E	1	Fine.
	82	2	0	82	68	30,27	41		ENE	1	Fine.
8	52	7	0	57	66	30,42	52		ENE	2	Cloudy,
	69	2	0	69	67	30,38	43		NE	1	Fine.
9	48	7	0	52	65	30,34	55		NE	1	Fine.
	68	2	0	67	67	30,31	42		E	1	Fine.
10	52	7	0	57	65	30,36	52		NE	1	Cloudy.
	67	2	0	67	65	30,35	43		NE	1	Cloudy.
11	47	7	0	53	64	30,39	52		NE	1	Fair.
	67	2	0	67	64	30,37	42		NE	1	Cloudy.
12	48	7	0	53	63	30,33	52		E	1	Fine.
	70	2	0	70	65	30,27	39		E	1	Fine.
13	53	7	0	58	64	30,23	53		S	1	Fine.
	78	2	0	77	67	30,21	36		SSE	1	Fine.
14	57	7	0	61	66	30,18	48		E	1	Fine.
	75	2	0	75	68	30,15	42		E	1	Fine.
15	56	7	0	61	67	30,05	55		E	1	Fine.
	72	2	0	71	67	30,00	40		E	1	Fine.
16	56	7	0	60	67	29,95	62		NE	1	Cloudy.
	74	2	0	73	68	29,93	42		E	1	Fair.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
June 17	56	7 0	58	67	29,91	63		ENE	1	Cloudy.
	77	2 0	76	68	29,89	40		NE	1	Fair.
18	56	7 0	60	68	29,89	59		NE	1	Fair.
	79	2 0	79	69	29,84	38		S	1	Fair.
19	58	7 0	58	68	29,75	58	0,097	S	2	Fair.
	62	2 0	59	67	29,71	54		W	2	Rain.
20	48	7 0	52	66	29,65	58	0,435	WSW	2	Fine.
	70	2 0	70	66	29,70	42		WNW	1	Cloudy.
21	54	7 0	54	65	29,73	54	0,105	WNW	2	Fair.
	63	2 0	61	65	29,78	55		WNW	2	Rain.
22	53	7 0	56	64	29,98	51	0,017	WNW	2	Cloudy.
	66	2 0	66	65	30,06	44		NW	1	Cloudy.
23	53	7 0	55	64	30,17	51		W	1	Cloudy.
	69	2 0	67	64	30,17	52		S	1	Cloudy
24	58	7 0	59	64	30,26	62	0,253	NE	1	Fine.
	75	2 0	75	65	30,23	42		NE	1	Cloudy.
25	57	7 0	59	64	30,13	62		SW	1	Fair.
	79	2 0	77	66	30,07	41		WNW	1	Fair.
26	59	7 0	61	66	30,03	59		W	1	Cloudy.
	76	2 0	75	67	30,01	42		WNW	1	Cloudy.
27	61	7 0	62	66	30,02	56		WSW	1	Fine.
	69	2 0	68	68	30,00	35		N	1	Fine.
28	61	7 0	62	68	30,03	48		WSW	1	Fine.
	86	2 0	86	70	29,98	32		SSW	1	Fine.
29	65	7 0	68	70	29,85	42		SE	1	Fine.
	83	2 0	81	71	29,77	40		S	2	Cloudy.
30	61	7 0	62	70	29,78	56	0,018	SW	2	Fair.
	81	2 0	81	70	29,82	41		W	2	Cloudy.

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1798	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
July 1	59	7	0	60	68	29,85	59	0,105	S	2	Cloudy.
	74	2	0	73	68	29,72	50		S	2	Fair.
2	55	7	0	58	68	29,62	52		S	2	Fair.
	69	2	0	62	67	29,63	54		W	2	Rain.
3	54	7	0	56	67	29,85	57	0,210	SW	1	Fine.
	76	2	0	76	68	29,83	46		SSW	1	Fair.
4	63	7	0	64	68	29,83	66	0,025	SW	2	Cloudy.
	78	2	0	78	69	29,83	44		SSW	2	Cloudy.
5	63	7	0	64	68	29,80	58		S	2	Cloudy.
	70	2	0	69	68	29,76	59		SSE	2	Cloudy.
6	59	7	0	60	68	29,78	63	0,396	SSW	1	Fair.
	73	2	0	71	68	29,79	45		SW	1	Fair.
7	58	7	0	61	65	29,77	59		S	2	Cloudy.
	73	2	0	73	67	29,76	43		S	2	Cloudy.
8	57	7	0	60	66	29,80	56		SSW	1	Fair.
	73	2	0	73	67	29,74	44		SSW	1	Cloudy.
9	53	7	0	57	66	29,83	58	0,083	SSW	1	Cloudy.
	71	2	0	71	67	29,86	52		SW	2	Cloudy.
10	61	7	0	63	66	29,85	71	0,102	S	1	Rain.
	73	2	0	73	67	29,88	57		S	1	Cloudy.
11	59	7	0	59	67	29,72	67	0,145	S	1	Rain.
	73	2	0	72	68	29,68	44		W	1	Cloudy.
12	53	7	0	56	65	29,73	58		SW	2	Fair.
	67	2	0	62	65	29,71	53		SSW	1	Cloudy.
13	51	7	0	54	64	29,83	64	0,083	S	1	Fine.
	70	2	0	68	65	29,80	50		SW	2	Fair.
14	56	7	0	59	65	29,48	67	0,150	S	2	Rain.
	70	2	0	70	66	29,56	41		SW	2	Fair.
15	52	7	0	55	65	29,67	57	0,078	SSW	2	Fine.
	69	2	0	68	65	29,64	46		S	2	Fair.
16	54	7	0	57	64	29,44	57		S	2	Fair.
	70	2	0	69	65	29,36	47		S	2	Fair.

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		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
July 17	52	7	0	57	64	29,38	60	0,025	NE	1	Hazy.
	69	2	0	67	65	29,49	50		NE	1	Hazy.
18	52	7	0	56	64	29,90	62	0,120	SW	1	Fair.
	74	2	0	74	66	29,96	38		W	1	Fair.
19	58	7	0	60	65	29,94	72	0,137	SW	1	Rain.
	75	2	0	74	67	29,94	50		S	1	Cloudy.
20	59	7	0	61	66	29,84	74	0,360	S	2	Cloudy.
	72	2	0	72	67	29,49	68		S	2	Rain.
21	53	7	0	55	66	29,58	60	0,110	SSW	2	Fair.
	71	2	0	69	66	29,64	46		SSW	2	Fair.
22	53	7	0	54	65	29,77	61		S	1	Fine.
	71	2	0	70	66	29,83	44		SSW	2	Fair.
23	52	7	0	56	64	29,84	62	0,140	SE	2	Fair.
	70	2	0	69	65	29,78	46		SSE	2	Cloudy.
24	56	7	0	58	64	29,63	64	0,035	SSE	1	Cloudy.
	72	2	0	69	65	29,64	53		SE	1	Cloudy.
25	58	7	0	56	64	29,78	68	0,365	SW	1	Cloudy.
	68	2	0	67	65	29,86	54		NW	1	Cloudy.
26	54	7	0	56	64	30,01	65	0,195	WSW	1	Cloudy.
	68	2	0	66	64	30,05	52		W	1	Cloudy.
27	57	7	0	60	64	30,11	67		WNW	1	Cloudy.
	72	2	0	72	65	30,14	52		WNW	1	Cloudy.
28	56	7	0	57	64	30,16	67		SSW	1	Cloudy.
	70	2	0	70	64	30,14	53		SSW	1	Cloudy.
29	60	7	0	59	64	30,13	69	0,015	W	1	Cloudy.
	71	2	0	71	66	30,16	42		N	1	Fair.
30	56	7	0	60	64	30,17	64		NE	1	Cloudy.
	74	2	0	74	67	30,10	45		S	1	Hazy.
31	56	7	0	58	65	30,02	63		S	1	Fair.
	75	2	0	75	72	29,97	42		SE	2	Fine.

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1798	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	c									
Aug. 1	61	7 0	64	68	29,86	55		E	1	Cloudy.
	83	2 0	82	70	29,85	42		SSE	1	Cloudy.
2	64	7 0	64	68	29,91	64	0,415	SW	2	Cloudy.
	73	2 0	73	69	29,98	45		W	2	Cloudy.
3	58	7 0	60	68	30,07	60		E	1	Fair.
	76	2 0	76	70	29,99	46		SE	1	Fine.
4	65	7 0	65	69	29,70	72	0,380	S	1	Rain.
	74	2 0	73	70	29,75	55		SSW	1	Cloudy.
5	56	7 0	57	68	30,11	62	0,125	NW	1	Fine.
	70	2 0	70	67	30,20	45		NW	1	Cloudy.
6	53	7 0	56	67	30,30	61		W	1	Cloudy.
	71	2 0	70	67	30,32	53		W	1	Cloudy.
7	56	7 0	57	67	30,30	60		NE	1	Hazy.
	73	2 0	72	68	30,26	47		NE	1	Cloudy.
8	61	7 0	61	67	30,17	62		E	1	Cloudy.
	67	2 0	66	67	30,11	52		E	1	Cloudy.
9	55	7 0	57	66	29,97	61		SE	1	Fair.
	76	2 0	76	67	29,94	51		SSE	1	Hazy.
10	58	7 0	58	66	29,86	64	0,063	SSE	1	Rain.
	68	2 0	66	66	29,80	60		SE	2	Cloudy.
11	54	7 0	56	66	29,97	67	0,072	WSW	1	Fair.
	73	2 0	73	67	30,03	45		W	1	Fair.
12	61	7 0	62	66	30,02	68	0,025	S	2	Cloudy.
	76	2 0	76	68	29,98	57		S	2	Fair.
13	61	7 0	64	68	29,88	68		S	2	Cloudy.
	80	2 0	80	70	29,82	50		S	2	Fine.
14	64	7 0	64	69	29,77	64	0,020	NNE	1	Cloudy.
	74	2 0	74	70	29,83	43		NE	1	Fair.
15	50	7 0	58	68	29,95	64	0,223	NE	1	Cloudy.
	72	2 0	70	68	29,98	46		N	1	Fair.
16	55	7 0	59	68	30,06	61		NE	1	Fair.
	70	2 0	69	69	30,08	50		E	1	Fair.

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	°										
Aug. 17	54	7	0	56	68	30.23	61	0,100	E	1	Fair.
	74	2	0	74	69	30.25	44		E	1	Fine.
18	55	7	0	58	66	30.22	61		E	1	Fair.
	74	2	0	74	69	30.16	44		E	1	Fine.
19	60	7	0	63	69	30.06	58		E	1	Cloudy.
	76	2	0	78	70	30.05	43		ESE	1	Fair.
20	59	7	0	61	69	30.25	61		ENE	1	Fair.
	75	2	0	78	70	30.20	44		E	1	Fine.
21	58	7	0	62	69	30.24	70		NE	1	Cloudy.
	75	2	0	73	71	30.21	48		E	1	Fine.
22	59	7	0	63	70	30.09	70		E	1	Fine.
	76	2	0	76	72	30.01	41		E	1	Fine.
23	62	7	0	64	71	29.80	61		NE	1	Cloudy.
	76	2	0	76	71	29.76	55		SSW	1	Cloudy.
24	56	7	0	58	70	30.03	56	0,102	SW	1	Cloudy.
	72	2	0	72	70	30.22	41		W	1	Fair.
25	58	7	0	62	68	30.21	62		SW	2	Cloudy.
	75	2	0	73	70	30.13	47		WNW	2	Cloudy.
26	52	7	0	55	68	30.35	58		NE	1	Fine.
	70	2	0	70	70	30.35	42		NE	1	Fine.
27	54	7	0	57	68	30.35	55		W	1	Cloudy.
	71	2	0	71	68	30.35	47		NNE	1	Cloudy.
28	62	7	0	62	68	30.33	59		N	1	Cloudy.
	71	2	0	70	69	30.32	47		N	1	Fair.
29	60	7	0	63	68	30.30	59		NE	1	Cloudy.
	73	2	0	72	70	30.25	43		E	1	Fine.
30	53	7	0	56	68	30.30	60		E	1	Fine.
	72	2	0	72	70	30.28	42		E	1	Fine.
31	58	7	0	58	67	30.34	55		ENE	1	Hazy.
	67	2	0	67	70	30.30	44		E	1	Fine.

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1798	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
Sept. 1	54	7 0	57	68	30,26	56		SE	1	Fine.
	72	2 0	72	69	30,18	44		S	1	Fine.
2	54	7 0	60	68	30,00	54		SSE	1	Fine.
	76	2 0	75	69	29,90	37		S	2	Fair.
3	60	7 0	63	68	29,76	64		S	1	Fair.
	74	2 0	73	69	29,70	55		SSW	2	Cloudy.
4	61	7 0	63	69	29,56	61		SW	2	Cloudy.
	73	2 0	73	70	29,70	43		WSW	2	Fair.
5	55	7 0	58	68	29,88	57		S	2	Cloudy.
	68	2 0	68	68	29,87	53		S	2	Fair.
6	50	7 0	51	67	30,01	59	0,035	S	1	Cloudy.
	64	2 0	64	66	29,97	49		SE	1	Cloudy.
7	54	7 0	54	66	29,85	60	0,028	NE	1	Rain.
	63	2 0	62	65	29,78	54		NE	1	Cloudy.
8	55	7 0	57	65	29,67	70	0,023	NE	1	Cloudy.
	65	2 0	65	65	29,66	56		NNE	1	Cloudy.
9	53	7 0	54	64	29,72	65		NE	2	Cloudy.
	68	2 0	68	66	29,65	58		ESE	2	Fair.
10	60	7 0	62	66	29,53	63		S	2	Fair.
	67	2 0	65	66	29,54	61		S	2	Cloudy.
11	53	7 0	53	65	29,51	68	0,475	SSW	2	Fine.
	67	2 0	65	66	29,49	49		S	2	Cloudy.
12	49	7 0	52	63	29,27	65	0,285	S	2	Rain. [Much wind last night.
	62	2 0	60	64	29,30	54		S	2	Fair.
13	49	7 0	51	62	29,16	70	0,270	SSE	2	Rain.
	60	2 0	60	63	29,32	61		NE	2	Fair.
14	53	7 0	55	62	29,90	66	0,112	N	1	Fair.
	64	2 0	64	63	30,03	59		NNE	1	Cloudy.
15	54	7 0	56	62	30,08	67		S	1	Cloudy.
	68	2 0	65	63	30,02	70		SSW	1	Cloudy.
16	55	7 0	55	63	30,06	68	0,043	WSW	1	Fine.
	67	2 0	66	64	30,13	46		W	1	Fine.

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1798	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
Sept.17	52	7 0	53	63	30,19	65		W	1	Fair.
	68	2 0	68	64	30,17	48		W	1	Cloudy.
18	52	7 0	53	63	30,20	68		W	1	Fine.
	69	2 0	68	64	30,19	52		SSW	1	Fair.
19	52	7 0	53	63	30,09	65		E	1	Cloudy.
	67	2 0	67	65	30,00	60		S	1	Fair.
20	53	7 0	54	64	29,96	67		E	1	Cloudy.
	68	2 0	68	65	29,82	55		S	2	Fair.
21	50	7 0	50	64	29,86	62		WSW	1	Fair.
	64	2 0	64	65	29,88	45		W	1	Fair.
22	48	7 0	52	63	30,03	63		WSW	1	Cloudy.
	64	2 0	64	63	30,05	56		SSW	1	Cloudy.
23	55	7 0	58	63	30,06	69		S	2	Cloudy.
	68	2 0	68	64	30,05	49		S	2	Fine.
24	59	7 0	61	65	29,95	73		S	1	Cloudy.
	70	2 0	70	65	29,99	55		S	1	Cloudy.
25	52	7 0	53	64	29,92	61		SSW	1	Cloudy.
	62	2 0	62	64	29,90	41		W	2	Fair.
26	48	7 0	51	63	29,94	62		SW	1	Cloudy.
	61	2 0	61	64	29,91	53		WNW	2	Fair.
27	50	7 0	51	62	29,24	67	0,115	S	1	Cloudy.
	55	2 0	52	61	28,97	66		W	1	Rain.
28	45	7 0	45	60	28,97	66	0,730	WNW	2	Rain.
	50	2 0	50	58	29,01	65		NW	2	Rain.
29	44	7 0	48	58	29,16	64	0,321	W	2	Fair.
	56	2 0	56	58	29,25	59		W	2	Cloudy.
30	47	7 0	47	58	29,55	69		WSW	1	Cloudy.
	58	2 0	57	59	29,73	59		SW	1	Fair.

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1798	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°										
Oct. 1	41	7	0	43	57	30,07	70		W	1	Cloudy.
	58	2	0	58	58	30,11	49		W	1	Fair.
2	52	7	0	54	58	30,19	66		WNW	1	Cloudy.
	61	2	0	61	60	30,26	56		N	1	Fair.
3	46	7	0	48	59	30,30	69		S	1	Foggy.
	59	2	0	59	61	30,30	62		NE	1	Fine.
4	52	7	0	54	60	30,16	71		NE	1	Cloudy.
	58	2	0	58	62	30,10	50		E	2	Fine.
5	49	7	0	54	60	29,88	66		E	2	Cloudy.
	58	2	0	57	61	29,83	65		E	2	Cloudy.
6	52	7	0	53	60	29,98	80		E	1	Foggy.
	59	2	0	59	61	30,07	71		S	1	Cloudy.
7	46	7	0	46	60	30,26	73		S	1	Fine.
	63	2	0	63	61	30,26	63		SW	1	Fine.
8	47	7	0	47	60	30,26	70		SW	1	Fine.
	62	2	0	62	63	30,26	62		SSW	1	Fine.
9	46	7	0	50	61	30,17	70		W	1	Cloudy.
	62	2	0	62	63	30,08	58		SSE	1	Fair.
10	53	7	0	55	62	29,88	77		S	2	Cloudy.
	64	2	0	62	62	29,83	68		S	2	Cloudy.
11	55	7	0	55	62	29,74	70	0,140	SSW	1	Cloudy.
	60	2	0	55	62	29,70	68		NW	1	Rain.
12	40	7	0	40	62	30,07	65	0,137	NW	1	Fine.
	52	2	0	51	60	30,19	52		NW	1	Cloudy.
13	32	7	0	33	58	30,39	62		W	1	Fine.
	47	2	0	47	60	30,35	45		S	1	Fine.
14	37	7	0	37	58	30,04	60		E	1	Fine.
	55	2	0	52	59	29,82	56		ESE	2	Fair.
15	52	7	0	56	59	29,63	82	0,052	S	2	Cloudy.
	60	2	0	60	62	29,81	51		S	2	Fair.
16	50	7	0	50	60	29,78	70		S	1	Cloudy.
	57	2	0	55	62	29,85	70		S	1	Cloudy.

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		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Oct. 17	47	7	0	53	60	29,92	73	0,158	SSE	2	Cloudy.
	62	2	0	62	62	29,92	70		SSE	2	Cloudy.
18	56	7	0	56	62	29,72	74	0,018	SE	2	Cloudy.
	58	2	0	56	62	29,64	72		SSE	2	Rain.
19	42	7	0	42	60	29,81	72	0,265	SW	1	Fair.
	56	2	0	56	63	29,89	52		WNW	1	Fair.
20	44	7	0	46	60	30,05	69		SSW	1	Cloudy.
	58	2	0	58	62	30,04	63		S	1	Cloudy.
21	54	7	0	55	61	30,07	73		SSW	1	Cloudy.
	60	2	0	60	63	30,08	60		S	2	Fair.
22	53	7	0	53	60	29,90	64		S	2	Cloudy.
	59	2	0	59	62	29,89	60		S	2	Cloudy.
23	48	7	0	48	61	29,94	73	0,055	SE	1	Fair.
	60	2	0	60	62	29,87	61		SE	1	Fair.
24	53	7	0	53	61	29,68	74		SSE	1	Fair.
	55	2	0	54	63	29,74	54		NW	1	Fine.
25	41	7	0	41	60	29,81	63	0,070	E	1	Cloudy.
	51	2	0	51	61	29,72	64		ESE	1	Rain.
26	45	7	0	46	61	29,58	74	0,172	S	2	Cloudy.
	62	2	0	62	62	29,53	66		SSE	2	Cloudy.
27	50	7	0	51	61	29,75	65		SE	1	Cloudy.
	51	2	0	50	62	29,68	72		E	1	Rain.
28	46	7	0	46	60	29,47	82	1,375	N	1	Rain.
	50	2	0	50	62	29,33	82		NE	1	Cloudy.
29	36	7	0	36	59	29,50	77	0,240	NE	1	Cloudy.
	52	2	0	52	61	29,44	67		E	2	Cloudy.
30	47	7	0	47	59	29,26	78	0,562	W	1	Rain.
	53	2	0	53	61	29,30	64		S	2	Cloudy.
31	46	7	0	46	59	29,71	69	0,185	SSW	1	Fine.
	55	2	0	55	60	29,78	60		S	1	Cloudy.

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	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Nov. 1	48	7	0	53	60	29,69	77	0,060	S	2	Rain.
	60	2	0	60	62	29,57	61		SSE	2	Fair.
2	54	7	0	54	61	29,37	73		S	2	Cloudy.
	58	2	0	58	62	29,37	71		S	2	Rain.
3	46	7	0	47	60	29,54	70	0,232	S	2	Fine.
	53	2	0	53	61	29,56	66		SSE	2	Cloudy.
4	40	7	0	40	58	29,83	67	0,041	S	1	Fine.
	49	2	0	49	61	29,92	62		SSW	1	Fair.
5	48	7	0	55	59	29,46	77	0,112	S	2	Cloudy.
	58	2	0	58	61	29,15	80		S	2	Rain.
6	41	7	0	41	58	29,39	67	0,157	SSW	2	Fine.
	49	2	0	49	60	29,44	57		W	1	Fair.
7	44	7	0	44	58	28,69	89	0,646	SW	1	Rain.
	47	2	0	47	59	28,83	74		NW	2	Cloudy.
8	43	7	0	46	58	28,77	81	0,155	S	1	Cloudy.
	53	2	0	53	59	28,72	68		S	1	Fair.
9	46	7	0	46	59	28,70	79	0,032	WSW	1	Rain.
	50	2	0	50	60	28,98	71		WNW	1	Rain.
10	40	7	0	40	57	29,43	70	0,128	W	2	Fine.
	47	2	0	47	60	29,55	60		W	1	Fine.
11	36	7	0	36	57	29,88	72	0,018	SW	1	Fine.
	45	2	0	45	59	29,98	63		SW	1	Fine.
12	37	7	0	37	56	30,01	70		SSE	1	Fine.
	45	2	0	45	59	29,96	64		SSE	1	Fine.
13	37	7	0	37	56	29,90	70		ENE	1	Fair.
	45	2	0	45	56	29,84	66		SE	1	Cloudy.
14	36	7	0	36	55	29,68	74	0,107	SSW	1	Fine.
	47	2	0	47	56	29,67	63		SW	1	Cloudy.
15	39	7	0	39	55	29,90	70	0,016	N	1	Cloudy.
	46	2	0	46	56	30,02	65		NE	1	Fine.
16	33	7	0	34	54	30,09	69		NW	1	Fine.
	43	2	0	43	56	30,14	65		N	1	Fine.

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	°			°	°						
Nov. 17	29	7	0	30	54	30,27	72				Foggy.
	36	2	0	36	55	30,25	71		SW	1	Fair.
18	25	7	0	25	52	30,15	73				Foggy.
	35	2	0	35	54	30,07	66		SW	1	Fair.
19	35	7	0	35	51	29,67	68		E	1	Cloudy.
	38	2	0	38	52	29,53	80		E	1	Rain.
20	33	7	0	33	51	29,85	74	0,420	NE	1	Fine.
	40	2	0	40	53	29,97	64		NE	1	Fine.
21	29	7	0	29	50	29,91	73		NE	1	Fine.
	35	2	0	34	52	29,75	65		E	2	Fine.
22	31	7	0	32	48	29,35	78		E	2	Snow.
	37	2	0	37	52	29,32	79		E	2	Rain.
23	35	7	0	40	50	29,36	82	0,391	E	1	Cloudy.
	40	2	0	38	52	29,38	76		E	1	Cloudy.
24	32	7	0	35	50	29,70	72		NE	1	Cloudy.
	42	2	0	40	52	29,80	71		NE	1	Cloudy.
25	25	7	0	27	50	29,88	75		W	1	Fine.
	36	2	0	36	51	29,83	74		W	1	Cloudy.
26	27	7	0	30	49	29,83	73		W	1	Fair.
	42	2	0	41	52	29,65	77		SW.	1	Cloudy.
27	40	7	0	50	50	29,19	93	0,302	S	2	Rain.
	53	2	0	48	54	29,10	78		SSW	2	Rain.
28	44	7	0	45	52	29,14	78	0,142	SSW	2	Rain.
	46	2	0	46	55	29,15	65		SSW	2	Fine.
29	38	7	0	38	52	29,37	75	0,075	SSW	1	Fair.
	46	2	0	46	54	29,47	69		WSW	1	Fair.
30	44	7	0	44	52	29,43	75	0,022	SW	1	Cloudy.
	46	2	0	46	54	29,44	67		W	1	Fair.

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		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°						
Dec. 1	35	8	0	36	52	29,74	74		SW	1	Fair.
	46	2	0	46	55	29,64	75		SSW	1	Rain.
2	43	8	0	43	54	29,43	76	0,058	SW	1	Cloudy.
	46	2	0	46	56	29,42	67		SW	1	Cloudy.
3	39	8	0	42	53	29,40	72		SSW	2	Cloudy.
	47	2	0	47	56	29,48	67		W	2	Cloudy.
4	34	8	0	35	54	29,93	75		W	1	Fair.
	40	2	0	40	56	29,97	70		E	1	Fine.
5	32	8	0	42	54	29,54	82	0,175	E	1	Rain.
	50	2	0	50	57	29,50	86		SE	1	Cloudy.
6	47	8	0	47	55	29,53	90	0,256	E	2	Rain.
	48	2	0	47	57	29,60	85		E	1	Cloudy.
7	42	8	0	42	55	29,90	83		E	1	Cloudy.
	43	2	0	43	57	29,90	82		E	1	Cloudy.
8	42	8	0	42	55	29,92	86		E	1	Foggy.
	45	2	0	45	56	29,97	83		E	1	Cloudy.
9	37	8	0	37	55	30,18	80		E	1	Fair.
	40	2	0	40	56	30,23	79		ENE	1	Fine.
10	34	8	0	34	54	30,26	68		ENE	1	Fine.
	41	2	0	41	56	30,14	53		NE	1	Fine.
11	31	8	0	32	51	30,05	62		E	2	Fine.
	38	2	0	38	53	29,98	60		E	2	Fine.
12	31	8	0	33	50	29,93	61		E	2	Fine.
	36	2	0	36	52	29,80	60		E	2	Fair.
13	29	8	0	30	48	29,54	65		E	2	Cloudy.
	31	2	0	31	49	29,52	63		E	2	Cloudy.
14	28	8	0	29	47	29,57	64		E	2	Cloudy.
	32	2	0	32	48	29,58	64		E	2	Cloudy.
15	30	8	0	34	47	29,41	84				Foggy.
	43	2	0	42	50	29,40	87		E	1	Rain.
16	41	8	0	42	48	29,49	93	0,137	S	1	Cloudy.
	46	2	0	45	51	29,51	93		S	1	Cloudy.

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1798	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
	°										
Dec. 17	44	8	0	45	50	29,36	95	0,147	SSE	1	Cloudy.
	48	2	0	48	53	29,27	93		SSE	1	Cloudy.
18	45	8	0	45	52	29,36	90	0,016	NW	1	Rain.
	46	2	0	46	53	29,47	84		NNW	1	Cloudy.
19	40	8	0	42	53	29,62	86	0,018	NE	1	Rain.
	42	2	0	41	54	29,80	77		NE	1	Cloudy.
20	31	8	0	31	51	30,26	73	0,020	NE	1	Fine.
	36	2	0	36	53	30,35	70		NE	1	Fine.
21	28	8	0	28	50	30,48	77		E	1	Foggy.
	36	2	0	35	52	30,46	68		SE	1	Cloudy.
22	32	8	0	35	50	30,22	68		S	1	Cloudy.
	44	2	0	37	52	30,12	77		SW	1	Cloudy.
23	37	8	0	37	51	30,26	77	0,030	E	1	Cloudy.
	39	2	0	39	51	30,33	68		ESE	1	Cloudy.
24	28	8	0	28	48	30,53	65		E	2	Cloudy.
	29	2	0	27	51	30,58	59		E	2	Fine.
25	24	8	0	24	47	30,30	70		NE	1	Fine.
26	26	2	0	26	47	30,28	64		NE	2	Fair.
	20	8	0	21	45	30,08	70		NE	1	Fair.
	22	2	0	22	45	30,01	66		NE	1	Fine.
27	14	8	0	14	42	29,76	67		E	1	Snow.
	18	2	0	17	43	29,59	67		E	2	Snow.
28	15	8	0	15	40	29,54	66		NE	1	Cloudy.
	17	2	0	17	43	29,74	62		N	1	Fair.
29	11	8	0	16	38	30,12	69		SW	1	Fair.
	32	2	0	32	42	30,18	72		SSW	1	Cloudy.
30	32	8	0	32	40	30,51	87		E	1	Cloudy.
	35	2	0	35	43	30,56	86		E	1	Cloudy.
31	28	8	0	30	41	30,39	79		E	1	Cloudy.
	34	2	0	32	43	30,34	80		E	1	Cloudy.

1798.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	53	28	39,6	53	29	40,1	59	49	53,5	30,52	28,96	29,94	90	73	82,8	1,105
February	54	24	39,9	54	24	40,1	58	49	54,2	30,76	29,23	30,11	90	71	82,2	0,693
March	58	30	42,9	58	30	42,9	61	50	55,4	30,37	29,18	29,93	90	68	79,8	0,333
April	69	30	51,6	69	31	52,7	66	53	59,8	30,38	29,27	29,96				0,517
May	76	43	56,5	75	46	57,3	66	58	60,6	30,44	29,11	30,00	69	30	51,4	1,621
June	86	47	64	86	51	64,8	71	61	65,8	30,42	29,65	30,07	69	32	50,1	0,960
July	78	51	63,9	76	54	64,4	72	64	65,9	30,17	29,36	29,80	74	38	55,8	2,879
August	83	52	65,6	82	55	63,9	72	66	68,5	30,35	29,70	30,09	70	41		1,525
September	76	44	58,9	76	45	59,2	70	58	64,2	30,26	28,97	29,78	73	37		2,437
October	64	32	51,8	63	33	52,4	63	57	60,7	30,39	29,16	29,90	82	45		3,428
November	60	25	41,6	60	25	42,4	62	48	55,4	30,27	28,69	29,58	93	57		3,056
December	50	11	35,2	50	14	35,5	57	38	50,3	30,58	29,27	29,90	95	53		0,857
Whole year			51,0			51,3			59,5			29,92				19,411

* The quicksilver in the bason of the barometer is 81 feet above the level of low water spring tides at Somerset-house.